

ON THE

ANTIDOTAL TREATMENT

OF THE

EPIDEMIC CHOLERA.

WITH A SKETCH OF THE PHYSIOLOGY OF THIS DISEASE, AS
DEDUCED FROM THAT OF INTERMITTENT FEVER

BY JOHN PARKIN,

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ROYAL COLLEGE OF PHYSICIANS, LONDON, AND LATE OF THE
HONORABLE EAST INDIA COMPANY'S SURGEON.

*"La Méthode tend à la guérison des maladies, et n'est pas en contradiction avec les recherches
dont elle se nourrit. Lorsqu'il s'agit de venir au secours du Choléra, le Médecin ne peut
rien que d'être humble." — Delpech.*

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TO

SIR RICHARD DOBSON, M.D.

ETC. ETC. ETC.

SIR,

Did no other feeling influence me, in the dedication of the present work, a sense of duty, alone, would have caused me to offer the first fruits of my professional labour to that individual under whom I had the good fortune to receive the chief part of my medical education. Although rejoiced at an occasion which enabled me, thus publicly, to acknowledge how sensibly I feel the obligations then conferred on me, I tremble lest, at the same time, it should appear how little I have benefitted by the opportunities I enjoyed—advantages which those only can appreciate, who have witnessed the results of your hospital practice—or had access to the valuable, yet unpublished, reports, of the numerous cases which have come under your care

during a long and honourable career of public duty.

Knowing how little you are disposed to hear from others the encomiums you so justly merit, I will only add, that while life remains, I shall remember, with no less gratitude than pleasure, the professional obligations and personal kindness you, for so long a period, have continued to bestow upon me.

Wishing you many years of health and happiness, and trusting that you may long continue to exercise, with the same benefit to others and credit to yourself, the skill and the talents which have already placed you so deservedly high in the profession to which we individually belong, I am, with every sentiment of respect and esteem,

Your obliged Servant,

And former Pupil,

THE AUTHOR.

London, May 10th, 1836.

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INTRODUCTION.

In presenting myself to the notice of my professional brethren, it would seem necessary to state shortly the motives which induce me, at this late period, to add another work to the number of those already published on this most prolific subject; and at a time, too, when the reign of terror of the Epidemic Cholera has nearly or entirely ceased. If, however, we glance at the history of all other Plagues and Epidemics, and if we observe the laws which this particular disease has obeyed in India since its first commencement, a period of 18 years, we shall be convinced that it is right to be prepared for the casual, if not periodical, return of the enemy, though we are not waiting in expectation of his appearance.

It cannot, therefore, be *superfluous* on my part, to endeavour to make known to the profession in general the result of a plan of treatment, which has claims on their attention, higher, in my opinion, than any other which has yet been employed in the cure of this most rebellious disease; for, although many remedies have been proposed as specifics, I am not aware that any evidence has yet been adduced which tends to show that, independent of the

medicine which will hereafter come under consideration, an *antidote* for the poison of Cholera has either been discovered, employed, or made known.

That we are in want either of a remedy, or a fixed and rational plan of treatment, is alike acknowledged and lamented by every sincere lover of his profession, and well wisher to the human race. Hitherto the treatment of this intractable malady has been the most uncertain, and the most empirical, of any which we find on record; and this, too, at a period, when we lay claim to something like sound therapeutical knowledge, and consider the science of medicine to be based on sure and fixed principles. In proof of this, we have only to call to mind the various methods, and opposite plans of treatment, which have been adopted by different practitioners for the cure of the disease, from its first appearance to the present day.

Thus, one individual, observing that the type of the disease assumed the form of that which is not uncommonly met with in tropical climates, Spasmodic Cholera, makes use of antispasmodics; and, meeting with some success, infers that spasm is the immediate cause of all the morbid phenomena, and, consequently, antispasmodics the principal, or only remedies required. A second meets with the disease under another form, that of sudden and immediate collapse, where evacuations from the stomach and bowels are almost entirely or altogether wanting. He resorts to the lancet, and finding it successful, immediately concludes that to venous congestion

may be referred all the symptoms present ; and that, therefore, venesection is the best resource. A third, remarking (more particularly in Europe) profuse evacuations from the stomach and bowels during life, being also the first symptoms which excite attention, and detecting, after death, what are considered signs of inflammatory action, locates the disease in the intestinal canal ; and directing his treatment accordingly, pins his faith on emollient drinks, leeches, &c. ; and if not a disciple of Broussais, adds to them emetics or purgatives.

These individuals, who were then treating the disease to the best of their ability, and according to the form which it assumed in these particular instances, should next have enquired if the same symptoms were always the most prominent and the most severe in every case ; or, if different remedies had not, in the hands of others, been equally successful, before they had ventured to generalize upon isolated facts. Had this been done, or had their followers first enquired into the varieties of each individual case, or ascertained the type of the disease, (for it will be found that the difference is more in places than persons) and considered what was the *modus operandi* of the medicines employed, something like a rational plan of treatment might have been adopted by the majority of the profession. But, instead of this, these different plans of treatment, adapted only to particular cases and particular periods, have been recommended as capable of curing the disease under every variety of form and

in all its various stages. And what has been the result of this? Others, without reflection, and led away by their reputed success, have followed the same methods under different circumstances, and with an opposite result. Hence the contradictory statements respecting certain remedies or particular plans of treatment; and hence the observation of one writer—that what seemed a remedy in White-chapel was no remedy in the Borough; and what was useful in the Borough, was useless in St. Giles'. Well might a talented reviewer exclaim, “Amidst the variety of remedies presented to our notice, we feel like a hungry guest, with a splendid bill of fare—each article tempts, but which shall he prefer? One gentleman cures Cholera with cold water—another removes it with hot—a third puts it to flight with calomel and opium—a fourth exclaims that calomel and opium are poison, and drenches with salt water—a fifth exclaims against the absurdity of salines by the mouth, and throws them into the veins—a sixth—but why should we go on?” It was certainly unnecessary to multiply examples, or to produce arguments in support of the observation that, in point of principles of treatment and practice, we were worse off on the commencement of the Epidemic in England, than we were during the first year of the existence of the disease in India.

And what else could be expected from a plan, or plans of treatment, whose only aim was the removal of effects, while the cause itself was left

altogether out of the question? As an example, I may mention the opposite results that have been obtained by blood-letting. In India—where this remedial agent was first resorted to, and where it was employed to the greatest extent—the success attending the operation was certainly such as to merit the encomiums bestowed upon it by most of the writers of that period. But then, what was the type of the disease in that country?—that of sudden and immediate collapse. Here then was a case where abstraction of blood, by removing the great plethora of the venous system and restoring in some measure the balance of the circulation, might be a powerful means in producing a determination of blood to other parts, of removing the poison from the internal organs, and distributing it to less vital parts. But, what was the result when the same practice came to be adopted in other climates, and venesection resorted to in the stage of collapse, when it had been preceded by premonitory diarrhoea of several days' duration, and by the profuse serous evacuations in the second stage of the disease; and when it was impossible to do more than empty the adjacent veins of the all but coagulated mass, without having the effect of producing any derivation of the poison itself from the internal and vital organs? Its almost universal abandonment, even by some of its most zealous supporters, is a sufficient answer to the question. So, again, as the type of the disease has been found to vary, not only in opposite climates but even in different places in the same country, we

find that the operation has been attended with success in one town, and with almost fatal results in others. I could also mention instances where this practice has been beneficial during one eruption of the disease, and prejudicial during another. The medical history of the malady, indeed, abounds with such examples.

To revert also to the injection of fluids into the veins.—As soon as it had been ascertained that the discharges from the bowels consisted of the serous part of the blood, combined with a certain portion of those salts naturally existing in this fluid, it was at once concluded that nothing more would be necessary to remedy the evil and remove the disease, than the injection into the veins of a certain quantity of water or serum, holding in solution similar salts. The momentary effect of the operation, it is true, was little less than magical; the patient being shortly restored from the state of a living corpse to that of an animate being. But, mark the result in the majority of instances; the poisonous matter was either carried on, by the force of the propelling fluid, to the brain, where it produced the same effect as had previously been witnessed in the thoracic and abdominal organs, viz. stagnation of the circulation and effusion of serum, and the patient died apoplectic—or else the poison was propelled to its former situation, when the evacuations became increased in quantity and frequency, the functions of the lungs again impeded, and the state of collapse more confirmed than before. This is only what might

have been prognosticated when means were taken to remedy an effect, without being able at the same time to remove the cause—the presence of a SPECIFIC POISON.

This, however, is not all. Puzzled by such contradictory reports, and thinking that nothing but chance would extricate them from the labyrinth, another class has adopted, instead of the former, which may be termed the symptomatic, the empirical treatment. But, enough ! I will not now attempt to enumerate the number of empirical remedies that have been resorted to, or waste the time of my reader in showing the injury which the science of medicine has sustained by this reprehensible conduct. It is confidently to be hoped that, after so much melancholy experience of the inutility of plunging head-long into the labyrinth of doubts by which we were surrounded, we shall henceforth belong to the class of rationalists and not to the class of empirics ; and that, if we cannot meet with any specific for the destruction of this poison, we shall, at least, treat the disease itself on the same just principles as are, or ought to be, our guide in the treatment of all other affections. And what are these principles ? Starting upon the well-known axiom, that there is no effect without a cause, let us first endeavour to ascertain to what agent the production of the disease is to be ascribed ; and then, having satisfied our minds on this point, direct our treatment to the removal of the cause, before we attempt to remedy

the effects of that cause ; and thus strike at the root of the evil, instead of wasting our time and our resources in lopping only the branches.

Having formed a theory respecting the Epidemic Cholera—the truth or fallacy of which is, at the present moment, immaterial ; and being in England on sick-leave during the eruption of the disease in this country, I was induced myself, and subsequently prevailed upon some of my friends, to administer the remedy which forms the subject of the present work, in the manner and for the object now pointed out ; being aware that it had been previously administered, if not in a different manner at least for a different purpose. It was not, however, until the severe eruption of the disease in London, in the summer of 1832, that I addressed the profession publicly on this subject. As the epidemic spread gradually over a considerable part of England, Ireland, and Scotland, subsequently to this, and from thence to America and Canada, (to all of which places I sent printed circulars recommending a trial of the different forms of Carbon) I was in hopes that much valuable evidence would have been obtained from these sources. But in this expectation I was, if not altogether, in a great measure disappointed ; for although it is certain, that, from the first commencement of the disease in England to its termination, carbonic acid came to be more and more generally resorted to and given, not only to check the vomiting and relieve the thirst, but in all periods

of the disease, including that of collapse itself, the evidence derived from this channel is only indirect.* Still much valuable collateral, if not certain, proof may be obtained even from this source—as I can state, from a comparison of many of the reports sent in to the central Board on the termination of the epidemic—that the most successful treatment appeared to be that to which carbonic acid had been added in some one form or other. This, so far, is an important link in the chain of presumptive evidence. If many different plans of treatment are adopted, and various remedies used, at the same time that a particular medicine is common to all, it is more philosophic to refer this effect to one particular cause than to many different ones, provided we have other proof that this cause is sufficient for the purpose. As an example, I may mention the different results that have been obtained with different individuals, by what has been termed the saline treatment. Dr.

* In justice to my professional brethren in England, I should state, that, in my circular to the different Boards of Health, as well as in the paper subsequently published in the "London Medical and Surgical Journal," I did not advise the exclusive use of carbonic acid. On the contrary, after giving a slight sketch of the theory I entertained respecting the antidotal properties of these substances, I attempted to explain the *modus operandi* of various medicines resorted to in this disease, as emetics, purgatives, mercury, &c.; and added, that they might, administered in a proper manner, be found useful and powerful adjuvants. Not having then had the same extended experience in the use of the remedy as at present, I did not feel justified in recommending others to deprive their patients of what might be considered the slightest chance, in a disease so rapid in its course and so fatal in its termination as that of Cholera Asphyxia.

Stevens, and several other practitioners, affirmed that this treatment was more successful than any other that had been adopted; while, on the contrary, many who had tried the same remedies considered them either useless, or worse than useless. Now, to what are we to ascribe this great discrepancy? Not to the perversion of facts or a biassed judgment; for, notwithstanding a dispute ensued, which became, to say the least of it, acrimonious and personal, we have no reason to question the honour or the motives of either party, much less of the talented individual most interested in the issue of the question. The solution of the problem I consider to be this:—It appeared, by the report of Sir David Barry, that the patients in the Cold-bath Fields prison were allowed an unlimited use of seltzer-water, to relieve the thirst and allay the irritability of the stomach; saline effervescing draughts being also added, to check the vomiting when this was severe—a practice followed to a certain extent by various other practitioners, who adopted the saline treatment. Well then might Dr. Stevens say, when asked by Sir David where his cases of collapse were, that his plan of treatment prevented the supervention of that stage of the disease, being well convinced that patients who had an unlimited allowance of soda-water, in the first stages of the disease, would never fall into a state of collapse. But as the exhibition of carbonic acid had no connection with the theory put forth by Dr. Stevens, and was not even mentioned as an adjuvant, in the first and original communications

of this gentleman, others who adopted the same plan of treatment did not add this agent to the saline remedies, and hence the difference in the result. It is also not to be forgotten, that as the carbonates, more particularly of soda, enter into the list of remedies constituting what has been termed the saline treatment, part of the success attending the practice may be ascribed, not to the action of the salt on the blood, but to the liberation of carbonic acid gas in the stomach, as I shall hereafter have occasion to remark.

I was about drawing up a table to show more clearly these facts, and to place in opposition the result of the administration of different remedies, either given alone or combined with carbonic acid, when the intelligence reached me of the extent to which the Cholera was prevailing in Spain. Believing that the remedy which I have proposed for the treatment of this modern plague, is a sure and certain antidote for the poison productive of the disease, and thinking that, possibly, the profession there would require ocular demonstration of the fact, before giving implicit credence to the assertion, in the then state of the question; being also anxious at the same time to obtain a more extended acquaintance with the disease, and, if possible, collect more direct evidence respecting the efficacy of the medicine, I was induced to pass over to that country. Prevented, by the political state of the northern provinces, from taking the shortest and most direct road by way of France, and thus meeting the enemy on its march, from south to north; and

detained several weeks in Cadiz, by the absurd and impolitic quarantine regulations ; my only resource was to take the circuitous route by sea to Valencia, visiting Alicante in my way, and from thence to Barcelona, where the disease had shortly before commenced—having extended northwards in two different directions, one from Seville, through the heart of the country intersecting Madrid ; and the other along the eastern coast, its point of departure being Granada, where it had been lingering nearly the whole of the previous winter. Having, in this journey, had an opportunity of giving the plan of treatment here pointed out a further trial, and finding my previous opinions and experience confirmed and strengthened ; at the same time that I have the satisfaction of being able to produce some valuable evidence, from several of the professors with whom I had the good fortune to be associated,—I no longer hesitate to submit to the consideration of the profession in general, the opinion I have formed as to the action of the remedy employed ; and to place before my brother-practitioners the facts, and the evidence which I have collected respecting its value and utility.

It is, however, with a feeling of the greatest deference and respect, that I thus venture to throw myself in the gap, to grapple with a foe with whom the most experienced and the most mighty have been unable to contend. But as the battle is not always to the strong, nor the race to the swift, it may be permitted even for one of the humblest keepers of

the human flock to try the strength of his sling against this modern Goliath, this ruthless destroyer of the sons of men. Neither will it, I am sure, weigh a moment with those anxious to sit in the light of day and not in the obscurity of darkness, that one of the least known of their fellow-workmen was the first to pick up the transparent crystal, which he found ready formed at his feet, the accidental production of a common fire, lighted for other and different purposes. I therefore rely with the most perfect confidence on the indulgence and liberality of those whose assistance I so much require, and from whom I hope to receive that support which the cause I am engaged in would seem to demand.

In the character of an humble but zealous labourer in the field of medical science, in the name of that profession to which we mutually belong, and for the sake of suffering humanity, I would ask, nay I would entreat, those who have the opportunity, to put to the test of experience the efficacy of this remedy. I ask this with the more boldness, inasmuch as it is a medicine which possesses no injurious property when introduced into the economy; and which may be given to almost any extent; while it is also to be remembered that no other remedy has yet been proposed capable of fulfilling a similar indication, much less evidence advanced in support of its efficacy. As I wish but for facts, and desire only the truth, I call upon those who have already given, or may hereafter give, this remedy a trial, to

make the result of that trial public, until such a body of evidence has been collected, that all doubt, all scepticism on the subject, shall be entirely removed.

Having, at a considerable sacrifice, and only by devoting myself almost exclusively to this disease for some years, endeavoured to make others acquainted with what I considered a valuable remedy, whose place was not to be supplied by that of any other, it is only fair, it is only just, that those who have the power should come forward and confirm my hopes, or banish my vain expectations. Having also ventured to state that this medicine is an unfailing remedy for the Epidemic Cholera, it is a duty which such individuals owe to those of their brethren who have *not* yet had an opportunity of witnessing the effect of its administration, to make their own experience public, confirm the fact, or bring forward evidence which will negative the assertion. This is an obligation at all times incumbent upon every member of the medical profession; but more especially when, as at the present moment, and in respect to this subject, he sees its honour, which he has sworn to protect, laid in the dust, the scoff and jest of the vulgar, the ignorant, and the prejudiced.

It is a duty, also, that those who profess to live not for themselves but others, owe to the public at large, in order that we may not again witness the reign of terror and despair, during future visitations of the Epidemic Cholera. Let the profession bring

forward satisfactory evidence that we possess a specific remedy for this disease, and that it is as capable of cure as any other, and we shall not henceforth hear of the rich and the affluent deserting their homes, and seeking refuge in the rocks and the mountains, far removed from domestic aid and professional assistance; neither shall we again have to record the fact, that those on whom the sick and the diseased are accustomed to rest their sole hope on this side of the grave, and who were endeavouring to arrest the progress of this mortal disease to the best of their ability, but without success, were themselves accused of the evil—of administering not a remedy but a poison—and immediately sacrificed to the blind fury and ungovernable rage of an unthinking and ignorant mob.

Shortly will be Published, by the same Author,

A TREATISE ON THE TREATMENT OF
DYSENTERY,

IN ITS ACUTE AND CHRONIC FORMS,

With Observations on the Remedial Virtues of the different Juices of
Corken in the Diseases of Tropical Climates.

ERRATA.

Introduction, page 14, under line 6—*the papers*, read *paper*.

Page 26, line 4—*the solution*, read *the fact*.

Page 27, line 20—*the method*, read *work*.

Page 30, line 14—*the equation*, read *condition*.

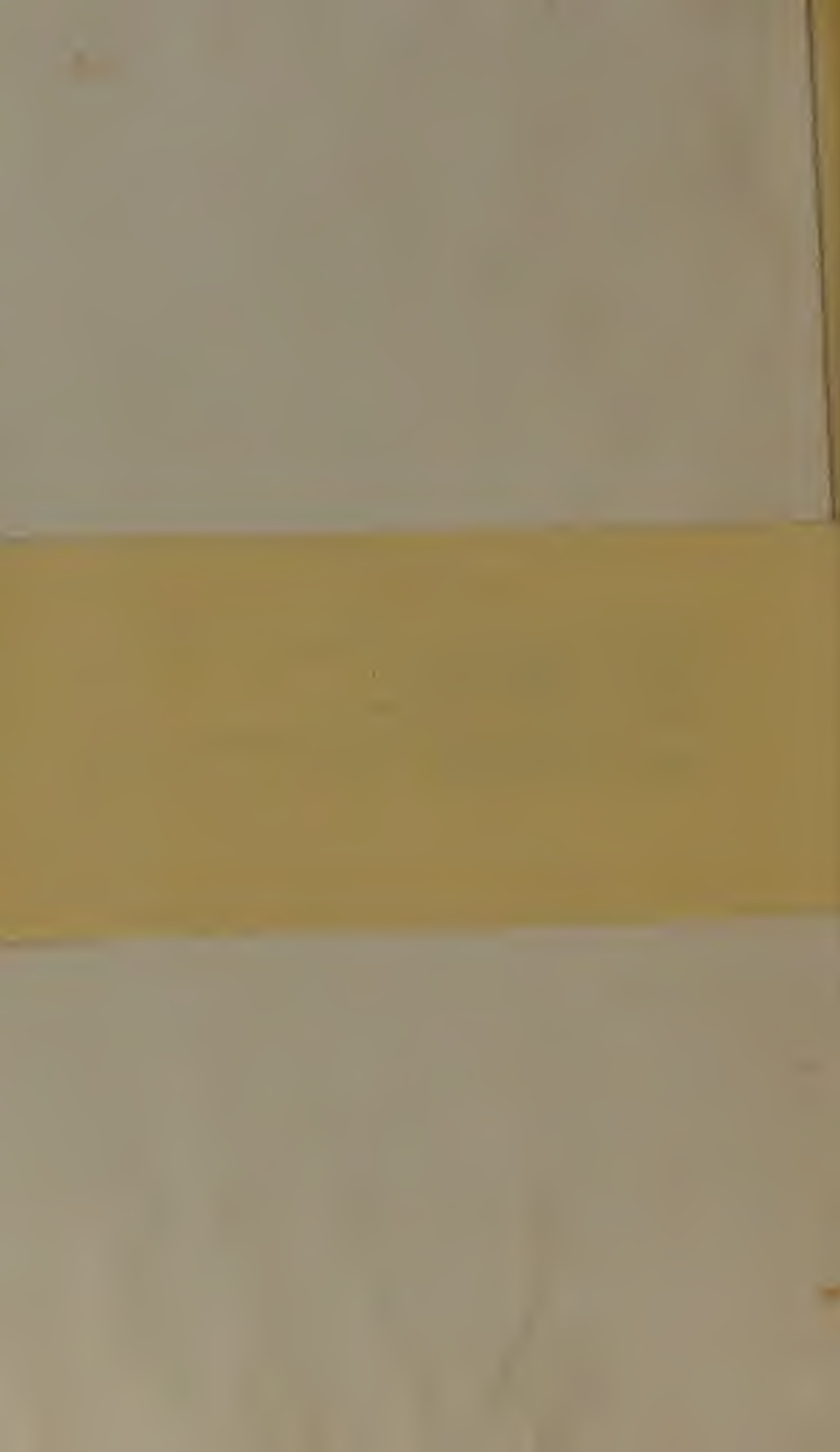
Page 35, under line 10—*the theorem*, read *theorem*.

Page 51, line 28—*the theorem*, read *lemma*.

Page 56, line 27—*the theorem*, read *lemma*, and *theorem*, read *lemma*.

Page 106, line 1—*the paper*, read *paper*.

Page 106, line 8—*the theorem*, read *lemma*.



CHOLERA.

SYMPTOMS.

As this disease is now, unfortunately, so well known, it would be superfluous to enumerate the symptoms by which it is characterized. It is necessary, however, to remark, for the perfect understanding of the observations about to be made, that I divide the Epidemic Cholera into two distinct and different forms;—the mild, which has received the generic name of *Cholerine*, and the severe, that of *Cholera Asphyxia*, or *Blue Cholera*. The latter, which alone forms the subject of the present work, I have subdivided into four different periods.

The premonitory* or preliminary *Diarrhœa*, so common and general a precursor of the other periods, forms with me the first stage of this disease. Again, that peculiar affection of the stomach cha-

* Premonitory, as Mr. Greenhow has remarked, is an erroneous term;—it is not a premonition of the disease, but a concatenation of circumstances constituting the early period of the disease itself.

racterized by malaise, giddiness, faintness, nausea, and vomiting of the contents of this organ, which sometimes precedes the Diarrhœa, but now generally follows it, and ushers in the next stage; and which forms, in those cases wherein the preliminary Diarrhœa is wanting, the first link in the chain of morbid symptoms, I have also placed in the same division.

I designate, as the second stage of the disease, that period of the attack when a fluid resembling serum, rice, or barley-water, is thrown up from the stomach, or evacuated from the bowels. Spasm may or may not be present at the same time, but the pulse is little if at all affected.

The state of collapse constitutes the third stage of the disease; and the consecutive fever the fourth.

PHYSIOLOGY.*

When we observe the symptoms which this singular malady presents, and compare them with

* Having taken with me to Spain the rough notes of a MS. which I was then preparing for publication, Dr. Garduqui, of Cadix, whose intimate acquaintance with the English language rendered him peculiarly fitted for the task, was kind enough voluntarily to undertake the translation of the same. This gentleman, whose zeal and interest on the occasion merit my warmest gratitude, was enabled, by devoting all the time he could spare from more active duties, to furnish me with a copy on my departure from that town for Barcelona, where it was published a short time after my arrival. In that work I connected myself with following, with some slight variation, the track marked out by Mr. Bell, in his views of the physiology of the Epidemic Cholera,—the more necessary at that time, in consequence of the opinion entertained by the author being almost entirely unknown in Spain; the generality of the profession there considering, with Bremsers, that the disease is, not a lesion of the nervous system, but an inflammatory affection of the mucous membrane of the intestinal canal. This memoir was subsequently translated into French by Dr. Dardel of Montpellier, and published about the period of the outbreak of the Cholera in the south of France,—an honour for which I am indebted to the favourable report given by the Academy of Medicine in Barcelona to that of Montpellier, in answer to their inquiries respecting the result of the administration of carbonic acid in that town.

In the above mentioned MS. some physiological remarks had been hazarded, as to the probable situation of the poison productive of the Epidemic Cholera, in the different stages of the disease. Having no facts, at that time, to bring forward in support of any hypothesis, the remarks alluded to were omitted, until further opportunities were afforded me of investigating the subject. During the following winter and spring, some clinical experiments were instituted for the purpose of deriving collateral evidence from the treatment then adopted; and having drawn certain conclusions from the result of these experiments,

those produced from various poisonous substances, more particularly septic ones; when those deductions are drawn which are allowable from the result of various methods of treatment; and when, as has been remarked by one writer,* all other theories which have yet been broached on the subject are insufficient to account for the morbid phenomena presented, the conclusion, that this disease is produced by the operation of a poison on the system, would seem as capable of proof as a mathematical proposition.

As, also, a number of persons, when congregated together in the same place, are always attacked simultaneously, we must infer that the cause is a general one, and that the poison is generated *without* and not *within* the body. Although we are at present in complete ignorance in what manner and where this deleterious agent is formed, when we witness the numerous changes which take place in the atmosphere at the commencement and on the termination of the malady; that the epidemic commences in one place on the setting in of particular winds, and subsides in another after heavy showers of rain, or the reverse of this, it is not illogical to infer that the poison is contained in the air we breathe.

both as regards the physiology and treatment of the Epidemic Cholera, I was induced to publish, during the last summer, a second part to the work in question; the observations contained in which will be found in the remarks about to be offered in the following chapter.

* Vide *Lancet*, "History of the Epidemic Cholera."

Taking it for granted, then, that the atmosphere is the vehicle for the introduction of the poison into the system, we may also infer that it first enters the lungs, and then passes on through the left side of the heart into the divisions and subdivisions of the larger arteries, until ultimately it reaches their extreme termination—the capillary vessels. But, although it enters the arterial portion of the circulating system, we cannot suppose that it remains long or to any extent in this situation. As we find after the collapse, when the patient recovers, that the attack is frequently followed by consecutive fever—and as this fever, there can be little doubt, is an effect of the same cause as that which produces the previous stage—we must conclude that, if the poison existed previously to the same extent in the same situation, it would produce similar results. It may, perhaps, be argued, that the consecutive fever is only an effect of the changes which take place in the composition of the blood, and a consequence of the loss of the greater part of the serous portion of this fluid. This cannot be granted, inasmuch as the fever is not an invariable consequence of the previous collapse; being frequently unobserved in persons who have had the disease in its severest form, where the discharge of serum has been the greatest, and when the collapse has existed for the longest possible period. We may therefore conclude, that the accumulation of the poison, after its first introduction into the human frame, and before it

produces any specific effect, does not take place in the arterial system. But if not in the arterial branches of the circulating system, where then does the poison accumulate? As there are but three divisions in this circle, if the poison passes into the circulating mass, and if it does not remain in the arterial, we cannot err much in inferring that the accumulation takes place either in the venous or the pulmonic system.

In intermittent fever, a disease produced by the poison termed malaria, whose existence we are certain of,—for, although not yet analysed or collected, its specific gravity is known, and the laws regulating its extrication or diffusion in the atmosphere are as well understood as the laws of gravity itself,—certain phenomena are presented so similar in every respect to those of Cholera Asphyxia, that we cannot fail to derive much collateral information from a consideration of the physiology of those diseases. Being certain that, in these cases, the poison is contained in the atmosphere, and that it enters the system with the air inspired, it is worthy of remark that no effect is perceived until the patient is suddenly seized with chilliness, shivering, and suspension of the circulation, either partial or entire; symptoms all referrible to derangement in the functions of the lungs or the heart. When, therefore, we witness the cold stage of ague, and observe that the paroxysm is followed by fever and other symptoms which denote derangement in the arterial system, did no other accession take place, we might

suppose, in this case, that the cold fit was produced by the first impression of the poison on the system, at the moment of its introduction into the body. But when the same morbid phenomena occur again and again at particular intervals, the generation of the poison, and the quantity contained in the atmosphere, remaining the same during the whole or a certain part of this time, we are compelled to search for another and a different explanation.

Inferring that, when the symptoms of fever subside, the agent productive of them has passed beyond the sphere of this morbid action, one of two results must follow; either the poison escapes by the exhalents on the surface, or else it passes on with the circulating fluid into the venous extremities. The latter is the only inference which can be drawn, when, in addition to what has been before remarked, we also remember that the accessions are frequently witnessed for a considerable time after the patient has been removed to a spot where the poison productive of these diseases, or, in other words, the diseases themselves, are entirely unknown. If, then, we suppose that the poison has been propelled into the extreme divisions or subdivisions of the veins, it is allowable to conclude that, from the operation of physical causes, it will be retained in the venous system for a longer period than in the former instance. Having, however, accumulated to a certain extent, and a given interval having elapsed, it may then pass on with the current of blood into the larger branches, until,

ultimately, it reaches the two veins which pour their contents into the right side of the heart. From this situation it may be carried forward by the impetus of the sanguineous current to the lungs; become arrested in the minute capillaries which traverse these organs; escape with the expired air, or enter the pulmonary veins, from whence it will be conveyed with the circulating mass into the left cavities of the heart, and ultimately become diffused in the arterial branches,—thus completing the entire circle of the sanguineous system.

Allowing, for the sake of argument, that the poison passes from the arterial into the venous system when the fever ceases, we will now endeavour to ascertain where this substance is situated when the accession commences. Finding that a certain period elapses, after the cessation of the fever, before any morbid phenomena are again produced, it is reasonable to conclude that, during this interval, the extraneous matter has travelled, with the current of venous blood, from the extremities to the centre. As it may be presumed that, in this common focus, the substance under consideration, arriving from the various branches of the venous system, becomes so concentrated that it will then produce effects not witnessed when separated and divided into minute portions, it was to be expected that the first effects observed in this disease were caused by the action of the poison on the heart or lungs. This supposition is confirmed by an analysis of the symptoms at the commencement of the acces-

sion, all of which are referrible to derangement in the circulating system and respiratory organs; but as the latter would seem to be more particularly affected than the former, and to be the first deranged, I have been led to infer that, in the cold stage of ague, the malarious poison, or at least the principal part of it, is situated in the pulmonary organs.*

If, therefore, we conceive that the malarious agent, having entered the venous system, has been attracted, by the operation of laws which seem to regulate the course of extraneous and other matters, from the extremities to the centre, we must also infer that to its detention in this situation, for a certain period, all the morbid phenomena then witnessed are to be ascribed. This effect may be in part due to mechanical causes; but the symptoms subsequently observed must be ascribed entirely to vital ones. Various interesting experiments have put us in possession of the fact, that substances of great viscosity cannot pass through the ultimate divisions of the arterial branches, but are arrested there, and occasion, not only the stoppage of the circulation, but the death of the individual. Inde-

* All extraneous substances in the blood, as Dr. Le Berri^{er} has observed, speaking of certain matters which have become introduced into the veins, come in contact with the pulmonary organ. Brochet, drawing the same conclusion, adds, that "we may consider each lung as a kind of sucking pump, which attracts towards it the greater part of the principles absorbed, without doubt to expel them to the exterior, or to subject them to a particular elaboration." But, as the former author remarks, the mechanism of the circulation sufficiently explains the phenomenon without having recourse to such an hypothesis.

pendent of various substances which possess great viscosity, such as oils, &c., the same result can be produced by gases, which are not easily miscible with the blood, or absorbable by water. If we ascribe such a property to malaria, and allow that the poison passes from the arterial extremities into the venous system, and from thence, through the right side of the heart, into the different branches of the pulmonary artery, we may then account for the stagnation of the circulation, and the other symptoms present in the cold stage of ague. This supposition,—that the poison of malaria does not pass readily through the capillaries of the lungs,—is not at variance with the conclusion before drawn, that the same matter traverses these very vessels on its first entrance into the body. The variation in the quantity of matter alone will fully explain the difference under these two circumstances, inasmuch as we have reason to believe that the accumulation of the poison in the system is very gradual; and that, consequently, it enters the lungs slowly and by degrees. Thus, many substances injected into the veins with a certain degree of force, and in a certain quantity, cause stagnation of the circulation, and subsequently death; but introduced slowly, and by degrees, so as to give time for their more intimate union and mixture with the blood, and to prevent the whole of the matter from entering the capillaries of the lungs at the same time, they do not produce any perceptible effect or morbid action. Atmospheric air, injected slowly and by degrees into

the veins, readily passes on in the blood without producing any inconvenience or fatal result ; but, when introduced suddenly, and in considerable quantity, it occasions the death of the individual. This is not unfrequently observed in man, after wounds in the neck and division of the jugular vein ; and in the horse, after venesection in the same situation ; when, from a momentary vacuum or other cause, the external air, during the act of inspiration, rushes into the veins and produces instantaneous death.

In order, however, not to allow this subject to rest on mere induction, I took advantage of my residence in Spain, where intermittent fevers may be said to be indigenous, to make some clinical observations, with a particular remedy, for the purpose of deriving collateral evidence on this subject from the result of the treatment then adopted.*

* As a duty, gratifying to me in the highest degree, I embrace the present public opportunity to return my humble thanks to the Spanish Government for the assistance rendered me on this occasion. Having visited a town in the province of Valencia, (after the subsidence of the Cholera in the autumn of 1834) where intermittents were prevailing to a considerable extent, but where, from want of proper facilities (most of the patients being in their own houses, and therefore under no control), I was unable to complete all my observations, — I made an application to the Minister of the Interior to allow me to take charge of some patients in one of the public hospitals. Her Majesty, in reply to this, having been graciously pleased to issue an order to the Supreme Board of Medicine, assigning me a ward in any of the hospitals I chose to name ; I selected the general hospital in Madrid, where these complaints always prevail, for reasons which I shall have occasion hereafter to explain when considering the cause of the production of intermittent fevers ; or, rather, the manner in which the poison is generated whereby they are produced.

Having concluded, with the generality of writers on this subject, that the poison termed malaria was the product of the putrefaction of animal and vegetable substances, it occurred to me, some years since, that carbon, and its various compounds, ought to be, in this case, an antidote for the matter, and a specific for the effects it produces, when introduced into the animal economy. The result of some experiments which I instituted, on a limited scale, having confirmed the opinion I had formed of the supposed value of the above agent, I eagerly embraced the first opportunity that offered of administering carbonic acid to some patients labouring under ordinary intermittents, but which had proved rebellious both to the bark and quinine. As, in these diseases, the stomach and bowels are altogether unaffected, we cannot refer any effect which may then be produced by the introduction of the remedy into the stomach, to any local action. As also this gaseous substance, when taken into the stomach and absorbed by the veins, usually passes out by the lungs without entering the arterial trunks (at least this is the case with the carbonic acid naturally existing in the venous system, and we have a right to infer that the same result will be obtained when introduced artificially), if any sensible effect is produced under these circumstances, we must conclude that the poison which is then acted on was situated either in the track of veins leading from the stomach and bowels to the heart,—in the

cavities of the right side of this organ,—or, lastly, in the ramifications of the pulmonic artery.*

In speaking thus, I would beg leave to be understood as referring to a remedy which, like carbonic acid, has no general or local action in the economy, and can only act indirectly in arresting any morbid effects—that is, by removing the cause; or, in other words, neutralizing the poison. The same cannot be said of quinine, which exerts a powerful action in the economy as a general tonic, and may thus enable the system to rouse itself, and eject the extraneous substance by one of those outlets destined for the discharge of excrementitious and other matters. This opinion receives considerable support from the fact that it generally takes some time to produce any change in the morbid action, even when the essence of the bark, quinine, is administered, and a considerably longer period when the bark itself is alone resorted to. That quinine, by its tonic properties, may sometimes enable the containing vessels to expel the morbid matter into

* In speaking of the situation of the poison, and its neutralization in the venous system, I, of course, take it for granted that absorption is admitted to be performed by the veins. The interesting and conclusive researches of Magendie leave no doubt upon this point; while the theory broached by this celebrated physiologist illustrates, most forcibly and clearly, the action of the remedy which we are now about to consider, and the cause of those apparently rapid effects, which will be presently detailed, at a time when we may conclude that the principal part of the poison is situated in the centre of the circulating mass, and in the great venous trunks.

other cavities, if not without the body, appears probable by the singular occurrence witnessed in Paris the winter after the fatal eruption of Cholera in that city. It must be familiar to many that, in the hospital of St. Louis, the administration of quinine was followed by the supervention of vomiting, purging, and all the other symptoms of a regular attack of Cholera. This having occurred more than once, and one of the cases terminating fatally, the attendant physician was induced to withhold the use of this remedy, and resort to others for the cure of the disease in those cases which subsequently presented themselves. A similar result was witnessed in the practice of Dr. Sauch, physician to the General Hospital in Barcelona; and, more particularly, in several parts of the south of Spain during the past year. In a severe epidemic which visited one town, the medical practitioner, having been accustomed to rely on this remedy at other periods, and not considering that the supervention of the above symptoms was to be ascribed to the quinine, still continued to persevere in its use. But, so regularly and so certainly did the same set of phenomena present themselves after the administration of the medicine, that this gentleman was at last convinced of its being the cause, and induced to relinquish the quinine altogether, trusting to general remedies for the cure of the disease.

To return, however, to the administration of carbonic acid in the usual form of intermittent fever. In order to determine the physiological points be-

fore discussed, this remedy was given at different periods :—in the intervals ; during the cold stage ; and in the fever stage.

1st. Administered on the intervening days of the accession—although, in some cases, the severity of the succeeding paroxysm was mitigated, and the disease itself, by a continuance in the same plan, subsequently cut short—yet, generally speaking, little or no perceptible effect was produced.

But when given about an hour, or an hour and a-half, before the expected period of the accession, repeating the dose every half hour until the cold stage had become fairly established, the attack was not only rendered much shorter and comparatively mild, but the succeeding paroxysm was, in ordinary cases, almost invariably prevented, provided the remedy was again administered in the same manner on the subsequent day of the attack. In some instances the disease appeared to be cut short at the moment, as the accustomed paroxysm was not witnessed.

2nd. In other cases, the patient commenced taking the remedy in the same manner, at the onset of the attack. The effect here was less marked than before—the duration of the cold stage being nearly the same as in the prior attacks ; but the severity of the fever stage was always sensibly diminished. The remedy being administered at the same period on the subsequent day of the accession, the attack was observed to be milder than before ; and the patient, by a continuance in the same plan,

was eventually restored to health ; but it was necessary to persevere with the remedy for a greater number of days than in the former instance, before being able to arrest entirely the morbid action.

With some individuals, on the other hand, in whom the cold fit lasted about an hour and a-half, the medicine was not administered until half an hour or an hour after the commencement of this stage. Little or no effect was observed, either in shortening the duration of the cold stage, or mitigating the intensity and continuance of the fever ; and although, by a repetition of the medicine in the same manner, the succeeding paroxysms were rendered milder and milder, unless recourse was had to the mode of administration pointed out in the second example, it generally required a long period before the regular return of the paroxysm was altogether prevented.

Again :—The supposed antidote was taken by several patients in whom previously the cold stage had been of very short duration, having lasted only ten or fifteen minutes. The medicine was ordered to be taken at intervals of half an hour, commencing with the first draught at the moment of the accession. From the short duration of the cold stage, only one dose could be taken before the fever stage had fairly set in. Little or no effect was produced, either on this or the following accessions, although the medicine was again administered in the same manner on the subsequent days of the attack. The remedy was then given in a different manner, the

patient commencing to take the medicine two hours before the usual time of the accession, repeating the dose every half hour until symptoms of fever presented themselves, or four or five doses had been taken. This alteration in the time of taking the remedy had the effect of preventing the accessions altogether in some cases, and of diminishing in a sensible degree the severity of the attack in the remainder; the disease itself being usually cut short after two, three, or four accessions.*

3rd. When administered during the hot stage, the good effect derived from the use of the remedy, introduced into the stomach, has been but little compared with the result obtained in some of the foregoing examples. In order, however, to prove the truth of my theory respecting the course of the poison in the body, and its situation in the different stages of the disease, I was induced to administer the gas in another manner, in the above stage, viz. by inhalation. Although limited in my opportunities of carrying on these clinical experiments, and notwith-

* In detailing these clinical histories, I would beg leave to be understood as giving the *general* result and not the *invariable* one. Many were the exceptions which I have met with to the above general rule, having witnessed favourable results in some cases, when the remedy was given at those periods in which, generally speaking, it produced no effect, and, on the contrary, I have had occasion to observe failures in other instances when it was administered at that period in which, in the majority of cases, a certain and favourable result was always obtained. As, however, my object in entering into a consideration of the physiology and treatment of these diseases is only with reference to an elucidation of the nature of the Epidemic Cholera, I shall reserve for a future occasion all observations on this point.

standing that the result cannot be looked upon as entirely conclusive without further confirmatory evidence on this point, I cannot help stating what I conceive to be a singular coincidence, as regards the similarity observed from the introduction of the remedy into the stomach during the cold stage, and after the inhalation of the gas into the lungs in the hot stage.

It has been shown that, when the remedy is introduced into the stomach, but little effect is produced unless given before that period when, as was inferred, the poison is situated in the capillary system of the lungs. So, also, when introduced into the arterial system in the commencement of the hot stage, the same good effects have been witnessed as when the gas has been taken into the stomach during the early period of the cold stage. But when the operation has been deferred, and the febrile stage fully established,—or, when it has existed for a certain period,—the amount of good effected has not only been considerably less, but in some cases almost entirely wanting. It cannot, therefore, be either an unprofitable or uninteresting question to ascertain to what this similarity ought to be ascribed.

The solution of this question will not, perhaps, present many difficulties, if it is allowed that the fever is an effect of the same cause as that which produces the previous stage, and that both are occasioned by a local action of the morbid matter ; that is to say, to its presence in the capillaries of

the lungs in the cold stage, and in those of the skin in the hot stage. This being granted, it is not an unimportant fact, that the result witnessed by the introduction of the remedy into the body in different ways, so as to be conveyed onward to the capillary system of the lungs, in the one instance, and to those of the skin in the other, has been the same; inasmuch as it shows that a certain state exists in these two different parts of the same system, and at these two different periods, common to each. Why it is that, when the poison is contained in the minute capillaries, so great a difficulty appears to exist of bringing the two agents now under review into complete approximation, is somewhat difficult of solution, owing to the imperfect state of our knowledge relative to the terminations of the capillary vessels. One class of physiologists contends for the existence of three sets of arterial capillaries; or, more properly speaking, three terminations of the capillary system, each devoted to a particular function, viz. one to nutrition, another to secretion, and a third and principal set to the transmission of the blood into the venous extremities. A second conceives that only one set of capillaries exists—those that terminate in, and convey the blood to, the ultimate divisions of the venous system; and that secretion, nutrition, and absorption are performed by means of lateral pores in the coats of these vessels. Now, without venturing to determine in favour of either the one or the other hypothesis (for it is not possible to prove the fact demonstratively),

I would only remark that we may, in either case, account for the failure of the remedy at those periods, supposing the poison to be situated in the capillary system. As it is certain that the lungs act as secreting and eliminating organs, by which not only the natural and component parts of the blood are separated and secreted, but extraneous matters also given off, and expelled from the system; and as it is equally well proved that the skin performs the same functions, and, like the lungs, gives off carbonic acid, it will be immaterial, as regards the effect of the remedy, whether these functions are performed by separate and distinct sets of vessels; or whether the substances in question are exhaled, by means of lateral pores, from those vessels which convey the arterial blood into the venous system. In either case, the remedy may escape without coming in contact with that portion of the poison which is contained either in the whole or a part of the capillary vessels which convey the blood into the venous system—in the former instance, because the deleterious matter is situated between the venous extremity of the capillary branch and the point from which the excretory branch is given off; and in the latter, because this matter is confined in the same set of capillaries, but beyond the part where the antidote escapes through the porous coat of the containing vessels.

That a part, at least, of this gas, when introduced into the arteries, escapes out of the body without entering the venous system, appears almost cer-

tain from the partial success which has attended the inhalation of the carbonic acid when employed during the intervals of the accessions. Had the remedy pursued the same course which it has been argued the poison takes in the circulating system, some result ought to have been witnessed when the antidote was introduced soon after the cessation of fever; unless we consider that the specific gravity of carbonic acid is greater than that of malaria, and that the remedy is longer performing the same circle in the system than the poison. As, in administering the gas by the stomach it is only necessary for the patient to take it about half an hour before the commencement of the accession, in order to bring the antidote in contact with the morbid matter; and as, therefore, during this period, it must have travelled from the extremities of the abdominal veins to the capillaries of the lungs, it shows that the passage of this gaseous substance in the venous system is very rapid. In fact, in some slight cases, in which it has been administered at the moment of the accession, I have seen beneficial effects produced in the short space of ten or twelve minutes; at a time, be it observed, when, from the shivering and other symptoms present, the poison had arrived, as we have reason to believe, in the pulmonary organs.

These results prove more forcibly than arguments, or logical reasoning, that the physiological deductions before drawn have some foundation for their support. Thus, when the remedy was ad-

ministered in the intervals, at that period when it has been inferred that the poison is distributed equally in the divisions and subdivisions of the venous trunks, and before the morbid matter has reached the track which the antidote takes from the stomach to the lungs, no effect has been witnessed ; as was to have been expected, if the theory before given is a true and correct one. Again, if any value is to be placed on the treatment adopted, we must also infer that the antidote, when given a short time previous to the accession, comes in contact with the poison ; and as this can only be in that part of the course which the remedy takes, from its entrance into the stomach to its presumed exit from the lungs, it leads at once to the conclusion that this extraneous matter, brought on by various channels from different parts, arrives about the same time at one common point or centre, in that situation where the main trunks of the different ramifications of the venous system are known to terminate.

But the principal question for consideration is, on what part does the poison act when it arrives at this common centre ? and where is this extraneous matter situated during the cold stage ? As it is evident that the poison has arrived at the termination of the two cavæ about the period of the commencement of the accession ; and as it appears that the situation of the malarious agent is different after the cold stage is fairly established ; we may conclude, what we should, *à priori*, have been led

to infer, that, during this interval, the extraneous matter has been propelled, with the vital fluid, to other and distant parts. Having before shown, that, when this medicine has been taken a short time previous to the accession, either the whole or the greater part of the poison has become neutralized; a similar result ought to have been obtained, if the situation of the poison at both periods had been the same, when the remedy was given in a like proportion in the cold stage; provided time was allowed for the administration and subsequent absorption of the antidote; and provided, also, that no other cause is in operation to prevent the union of these two agents during the cold fit. Making every allowance for the depression of the nervous energy and the lenthitude of the circulation, which may render the absorption of the remedy less prompt and less certain, at this particular period, in the severer cases; yet as, in others, where, from the mild type of the disease, no impediment could be offered to the absorption of the remedy, a nearly similar result has been observed, we must conclude that the non-neutralization of the poison, during the cold stage, is owing to another and a different cause. I am myself led to infer, that to locality alone is to be ascribed the difference observable in this respect; and which is to be explained by the supposition, that the poison has arrived, when the cold fit commences, in the pulmonary organs—an inference previously drawn both from analogy and an analysis of the symptoms at this period. If,

then, we conclude that the poison is contained in the capillaries of the lungs, and consider that the remedy escapes in part, if not wholly, so soon as it enters the one extremity of these vessels, without passing on to their extreme terminations, we have a satisfactory explanation afforded us why the whole of the morbid matter is not acted upon by the antidote during the cold stage; or, more properly speaking, why it is not neutralized to the same extent in the one case as in the other; for it is not always possible to neutralize the whole of the poison, during one accession, even under the most favourable circumstances.

Having arrived at the above conclusion, namely, that the poison of malaria is contained in the pulmonary organs during the cold stage of ague; and believing that sufficient has been already advanced to fulfil the object intended by the foregoing observations, viz. to illustrate more clearly the physiology of the Epidemic Cholera, we may now pass on to a consideration of the morbid phenomena presented to us by this disease.

The similarity of the symptoms, in the cold stage of ague and the collapse of Cholera, has been remarked and commented on by several writers; but no one has yet pointed out the *cause* of this resemblance, or satisfactorily accounted for the production of the morbid phenomena witnessed on these occasions. Although the collapse of Cholera, unlike the cold stage of ague, does not form the first link, generally speaking, in the chain of morbid symptoms

—yet, it being at this period that the relationship which these two diseases hold to each other is most apparent, and as it is the point to which all the previous observations were directed, and the one at which the foregoing remarks terminated—it will be advisable, first, to endeavour to elucidate the physiology of the stage of collapse, leaving for subsequent consideration the cause of that disparity which is produced, in the one instance, by the regular return of the paroxysm, and, in the other, by the supervention of vomiting and purging.

That, in the collapsed stage of Cholera, the poison productive of this disease is in the same situation as in the cold stage of ague, may be inferred from the phenomena which present themselves to our notice in the former disease. The first symptom witnessed in the commencement of collapse—the coldness of the extremities—shows that one of the most important functions of the lungs, the generation of animal heat, has been already suspended; and this, too, at a time when the circulation has not been as yet entirely arrested, and when other vital functions are not proportionately affected or depressed. It would seem, therefore, that we ought to look, for an elucidation of this phenomenon, to some local cause, besides the general one referrible to the depressing influence of the poison on the whole of the nerves of the grand sympathetic. The vain and fruitless efforts which, at a later period, the patient appears to make to fill his lungs with air; the quick and convulsive expiration, and the

choleric voice, all lead to the belief that the pulmonary organs are at this time in a state of partial or complete collapse. The contraction and compression of the *alælnasi*, which remain undilated even during the act of inspiration, also afford external evidence of the vacuum within.

But it may be answered, that the elevation and contraction of the chest still continue to the last moment of the patient's existence. Although this is the fact, it does not follow but that the circulation is, at this very time, altogether suspended, or the lungs themselves in a state of complete collapse; it having been satisfactorily proved, by the researches of Sir Charles Bell, that, for the right performance of this function, the integrity of the upper portion of the spinal marrow is alone necessary. Other experiments, in the hands of Sir B. Brodie, have also shown, that when those poisons have been introduced into the system which act directly on the heart, and suspend the circulation of the blood, the muscles which elevate and depress the thorax have still continued to perform their office for some time after the action of the heart has entirely ceased. That the same phenomenon is witnessed in Cholera, and that the medullary nerves are unaffected in the asphyxial stage of this disease, there can be little doubt; but then it becomes a question whether the arrest of the circulation is due to primary or secondary causes; to the direct action of the poison on the heart and lungs, or to the deleterious effect of the same agent on the whole of the nerves of the

grand sympathetic. Without wishing to lessen, altogether, this general effect, I am now only anxious to show that, from certain causes, the poison productive of this disease having accumulated in one particular part of the circulating system, in addition to the general, there is also a local effect produced by the presence of this deleterious agent in the minute capillaries of the pulmonary organ. That the poison is to be found in the vessels of the lungs, in the stage of collapse, will be rendered evident, if, in addition to the arguments before used, it is also allowed that the stagnation of the circulation is not caused by the depressing influence of the poison on the nervous filaments distributed to the heart and arteries. That the heart is often found beating distinctly and regularly, although slowly and feebly, even in the most severe and protracted cases, is beyond all dispute; and was particularly observed in the eruption at Madrid, when not only palpitation of this organ, but unusual pulsation in the abdominal aorta, were set down, by the observers of the epidemic in that capital, as pathognomonic symptoms of the disease. By applying the ear or the stethoscope to the chest, I have often convinced myself that the heart was beating distinctly long after the pulse at the wrist had ceased to be felt. We must conclude, therefore, that the arrest of the circulation is not entirely produced by the paralyzing effect of the poison either on the whole of the nerves of the sympathetic system in general, or on those of the heart in particular. Neither can we suppose that this pheno-

menon is the effect of inspissation of the blood, rendered too thick for pulmonary circulation by the previous escape of the serous part; inasmuch as the cessation of the pulse, and the arrest of the circulation, are often witnessed in cases of sudden collapse, and previous to the occurrence of any serous or other discharges. We may therefore conclude, that the disturbance of the lungs, in the commencement of collapse, is primary and not secondary; and that the arrest of the circulation is an effect, and not a cause, of the suspension of the functions of these organs. But as in this situation, in addition to the ganglionic, there are also distributed the branches of a cerebral nerve, it becomes a fair question to ascertain whether, supposing the poison is contained in the lungs, this circumstance can have any effect in the production of the phenomena now under consideration. In order to determine this, we have only to refer to what takes place when the lungs are deprived of the nervous energy derived from this source, by the division of the trunks of these nerves.

The section of the eighth pair of nerves—respecting which many observations have been made of late by different physiologists, more particularly Mr. Wilson Philip—produces two distinct and different effects; one on the larynx, and the other on the lungs. By the former, the opening into the glottis becomes so narrowed, that death often supervenes immediately. This arises, as has been pointed out by Magendie, from the section being performed

below the branches distributed to the constrictors, and above those given off to the dilators—in consequence of which these last become paralysed, while the former retain their contractile power—and hence the closure of the rima. When, however, death is not the immediate result of the operation, the animal may live for four or five days, but never beyond this period; during which time other phenomena are observed, referrible to the pulmonary organs. The respiration becomes more and more affected, according to the time which has elapsed since the animal was submitted to the experiment; inspiration is performed with difficulty and only by unusual efforts, while the expiration is quick and convulsive; the arterial blood gradually loses its vermilion tint, becoming of a darker shade, and similar in colour to that of the veins; the arteries themselves being, as has been discovered, nearly or entirely empty. Less and less oxygen is consumed by the animal; carbonic acid is not given off in its usual quantities from the lungs; animal heat is not generated; the temperature of the body becomes lower and lower; and death closes the scene. The inspection of the body, after death, discovers the pulmonary organs distended with thick black blood, and other signs of stagnation of the circulation in the capillaries of the lungs; to which effect may, perhaps, be ascribed the venous congestion, and emptiness of the arterial trunks.

As it would be foreign to my present purpose to enter into the question respecting the immediate

cause of death in these cases—a point not yet decided—I shall content myself with calling attention to the similarity of the effects produced by the above operation to the morbid phenomena presented in the collapse of Cholera. As the deprivation of nervous energy, by cutting off the supply, appears to have so great an influence on the functions of the lungs, we have only to allow that the choleroid poison acts injuriously on the eighth pair of nerves, and that it is contained in the lungs during the stage of collapse, to account for many of the symptoms which are then present. That the cerebral nerves are not insensible to the deleterious action of this poison, it will be my endeavour to show hereafter, in considering the nature of the consecutive fever. If, therefore, it is granted that the poison of Cholera acts injuriously upon the nerves of the cerebrum; and if we conclude that the symptoms characteristic of the stage of collapse are, in part, due to the loss of vital energy in the nervous filaments of the eighth pair distributed to the lungs, we have another and a strong confirmation of the truth of the proposition before made, viz. that the choleroid poison is situated in the pulmonary organs in the above period.

Resting satisfied with the foregoing observations respecting the situation of the poison in the collapsed stage, we may now revert to those differences between the Epidemic Cholera and the common form of intermittent fever which have been before alluded to, and which, at first sight, would

appear to negative the conclusion that the physiology of these two diseases is the same. These differences, however, upon a closer examination of the subject, will be found to be only varieties which each of the two diseases presents at different times and in different persons. As the common form of intermittent fever, for the reasons before mentioned, seemed most adapted for a trial of the remedy resorted to; and as the deductions given have been principally drawn from the result of that trial, no allusion has yet been made to other and different forms of the same disease. It will be necessary now, however, to consider what is called the malignant intermittent, which presents certain phenomena not witnessed in ordinary attacks.* In these malignant cases, the intensity of the cold fit is much greater, and the duration of the stage much longer; while, in addition to the usual symptoms, others are present peculiarly characteristic of the collapsed stage of Cholera. *The patient, after a certain interval, becomes cold as marble; the pulse ceases to be felt; the respiration is quick, and appears to be performed*

* The malignant ague is a disease very prevalent in the middle and southern parts of Spain, where it is to be met with, to a greater or less extent, every year. The malignancy and extent of these affections vary much in different years; but as they prevail principally in the autumn, we may say that their prevalence depends partly on temperature—with this peculiarity, that as, in other climates, we witness at one and the same time, or in succession, (according to the locality and temperature) continued, remittent, and intermittent fever,—in Spain we principally observe the malignant and common intermittent; both remittents and continued fevers being comparatively rare.

with difficulty ; the voice is weak, shrill, and hoarse ; the eyes are hollow and sunken ; the face becomes of a leaden colour ; the extremities livid ; and the secretion of urine entirely suspended. To complete the picture, and render the comparison perfect, *the mind, during this time, is unaffected,* and the strength sufficiently great, that, to use the words of Francisco Torti, “ if the patient wishes to get out of bed, he can accomplish it with ease.”

Finding, therefore, that, as regards the symptoms, there is no perceptible difference between the cold stage of the malignant ague and the collapsed stage of Cholera, we may more easily understand the cause of that variety in the two diseases which is presented by the regular return of the paroxysm in ordinary intermittents. This phenomenon, in all probability, is produced by the difference in the quantity of the poison in these several instances ; by which, in the one case, not only are all the functions of the lungs wholly suspended, but the circulation also in the minute capillaries entirely arrested, so that the poison is unable to pass on into the arterial system ; while, in the other, from the still partial flow of the sanguineous current, this effect is accomplished. As, however, this object is not effected without some difficulty, nor until after a certain interval, the duration of the poison in this situation gives rise to those phenomena witnessed in the cold stage of ague ; the intensity varying in different individuals, according to the amount of morbid matter which may be present.

Whether this effect is due to vital or mechanical causes, is not a matter of much importance at the present moment; but that the above circumstances are alone sufficient to account for the variety observed in these respective instances, may be inferred, independently of analogy, from the singular circumstance that, in the severe and malignant cases, the paroxysm is seldom followed by a second attack—the termination of the disease, like that of Cholera Asphyxia, being either in death or speedy and perfect recovery. This well-known fact is not a little remarkable, and affords another proof that the action of these two poisons is the same in the animal economy, and that the difference observed is only a difference of degree; while the varieties presented in these two cases are solely to be ascribed to the greater or less concentration and power of the morbid agent.

Not only may we ascribe the differences observed in these respective diseases, and in the different forms of the same complaint, to the greater or less concentration of the poison in the system; but, we may also attribute the greater amount of morbid matter, in the one case than in the other, to the more rapid generation of the poison, or, at least, to its evolution in the atmosphere. That the poison of Cholera is diffused in the atmosphere with great rapidity, there can be little doubt; for, although generally preceded for some weeks by a milder form of the same complaint, or Cholerine, yet in numerous other instances it breaks out, in its severest form, without

the least warning. So again, the period for the diffusion of this matter in the atmosphere, in a state of great concentration, seems to be very limited in the generality of cases ; the disease not continuing, in some situations, for more than two or three weeks, in its aggravated form. But as, in certain localities, the generation of the poison has been more gradual, and its diffusion in the atmosphere protracted to a more lengthened period, it may be asked,—if the course of the poison in the Epidemic Cholera is the same as in that of common intermittents, and if the variation in these two diseases is only caused by the difference in the quantity of morbid matter and the greater rapidity with which it is imbibed into the system, why does not the poison of Cholera, in other instances, produce the same effects as are witnessed in intermittent fever?—To this it may be answered, that such would appear to be the case ; for not only has the Cholera been preceded, accompanied, and followed, in a large portion of its pestiferous track, by intermittent fever, in places where these diseases are endemic, but in situations also where previously they were either unknown, or had not been witnessed for many years. This must be familiar to every person acquainted with the medical history of this epidemic ; while I may add that, in Spain, where this form of disease is so prevalent, intermittents were never known to prevail to so great an extent as subsequent to the appearance of the Cholera in that country. Alicante offers a singular and remarkable example in verification of the

foregoing remarks. In this town, placed somewhat out of the principal line of march which the disease took, from south to north, the epidemic lingered for about two months without occasioning the same ravages as in several other places situated on the above route—some of which were almost depopulated by this modern scourge of the human race. During this period the malady was confined entirely to the town, the houses and villages in the immediate neighbourhood remaining altogether exempt. In consequence of this, many of the inhabitants who fled from Alicante on the first outbreak of the disease, remained in the adjoining villages, until the subsidence of the epidemic. But although they escaped the Cholera, it is a fact, interesting in the highest degree, as regards the present subject, that nearly all those individuals were attacked with intermittent fever. In a second visit which I paid to this town, some months afterwards, an opportunity was afforded me of attending several of these patients; and having administered the remedy which has been already referred to, I had the satisfaction of removing the complaint in a short time, after the bark, quinine, and other remedies usually resorted to on such occasions, had entirely failed. A small island, situated about two miles from the town and coast, and to which many were induced to fly on account of the facilities it offered for cutting off all communication, suffered more severely than other spots; few who remained there having escaped without contracting a rebellious ague. Thus it was

that the town of Alicante formed a single and particular focus for the disease, the point of concentration of the poison; while around, within a certain distance, and beyond a circumference of half a league, the dilution was such that the exciting cause was unable to produce the malady in its severest form; and another and a different complaint was witnessed.

The same, or a similar, phenomenon occurs in many of the pestiferous spots of intertropical climates, when malaria is generated to any great extent. Those residing near the source where this poison is extricated, are attacked with continued fever in its highest grade; those at a greater distance, with a milder form of the same complaint, or remittent; while others, situated on the heights above, where distance and temperature both act in concert, are only affected with the common form of intermittent fever.

In Madrid also, where, from its dry and elevated situation, the prevalence of such a complaint is the more worthy of notice, intermittent fevers have been unusually common during the last two years. So much had they become generalized, indeed, in the spring of 1835, that one physician remarked to me, in his opinion, Cholera would not again appear that year; similar exemptions having been witnessed in other places, when intermittents prevailed to any considerable and unusual extent. Although this prognostic proved to be correct as regarded the re-appearance of the disease in an

epidemic form, it was not literally true in other respects, for several isolated cases occurred of real Asiatic Cholera; thus showing, that the same cause was in operation as in the preceding year, but not to a sufficient extent to affect the mass of the population. If, however, it could produce the disease, even in one individual, more particularly predisposed to be acted on by the morbid cause; we may conclude that it must have been producing minor, although perhaps different, effects in others, not so susceptible, but yet not entirely proof against its injurious impression. As, therefore, intermittents were the prevalent, indeed almost the only affections witnessed at this period, it may not be unwarrantable to infer, that these complaints were effects of the same cause as that which produced the Epidemic Cholera. This conclusion receives support from the fact, that the town before referred to, as I was informed by the senior practitioner, had been almost entirely exempt from ague for a period of twenty years,—an exemption in a great measure applicable to the surrounding neighbourhood, the disease being only known to a very limited extent in the Huerta, or small plain situated about a league from Alicante. When I add, that intermittents commenced in this town, after the subsidence of the epidemic, and continued to prevail to a considerable extent during the whole of the winter, we can hardly fail to infer that the epidemic influence was, in some way, concerned in their production.

We have now to consider the cause of another variety which exists between the phenomena presented by patients labouring under these respective diseases. The serous evacuations from the stomach and bowels, constituting the second stage of the Epidemic Cholera, are here alluded to. This variation, however, is only applicable to the common and ordinary form of intermittent fever; for, in the malignant ague, there is not only a state of Asphyxia, presenting the same symptoms and the same phenomena as those of the collapse of Cholera—and, like it, lasting an indefinite period instead of a certain portion of time,—but there also supervene vomiting and purging, of a clear, transparent, or conjee-looking fluid. In the common intermittent fever, the circulation of the blood during the cold stage is not entirely arrested; but in the more severe and aggravated form of the same complaint, this is found to be the case. There must necessarily be, therefore, in this disease as in Cholera, a stagnation of the venous system and plethora of the abdominal veins,—which, in consequence of being unprovided with valves (as Mr. Bell has explained) will receive a larger quantity of fluid than they are accustomed to do in a state of health. This, added to the greater and general deprivation of nervous energy in the grand sympathetic, and the consequent debility and want of tone in the mouths of the exhalents, may in part account for the super-vention of vomiting and purging in the severer cases, and their absence in the others. But we

cannot ascribe the vomiting and purging which occur in these malignant agues entirely to the stagnation of the blood, and the effect of the poison on the nervous system in general; inasmuch as, in these diseases as well as in Cholera, we sometimes have patients presented to us in whom every function under the control of that system of nerves is entirely suspended, and no such effect is observed. We must therefore look to local as well as general causes, for an explanation of this phenomenon. Now it is evident that, if the poison productive of these diseases passes into the arterial system upon its first introduction into the body, and from thence into the venous extremities, a portion of the same matter must traverse the arteries and veins of the intestinal canal, in common with those of other parts of the frame. As also, in the malignant cases, a greater quantity of poison must have accumulated in the system, to produce the disease in this aggravated form; by a parity of reasoning, a larger proportion of the same matter must pass through the abdominal vessels and the liver—to which may be ascribed, in conjunction with the stagnation of the circulation, the symptoms generally witnessed on these occasions. But as we sometimes see, even when the intensity and duration of the collapse are equal, that one patient will be attacked with vomiting or purging, and another not; quantity alone, as before remarked, does not account satisfactorily for the supervention of these symptoms. It must also be in part owing to accidental circumstances,

or particular idiosyncrasies; by which the poison becomes more readily determined to one organ than another, in different individuals and under different circumstances.

The same reasoning which has been used with regard to the supervention of vomiting and purging, in these cases, will also be applicable to the premonitory diarrhœa of Cholera. Although, in most cases of ague in which there has been the least tendency to take on a malignant form, diarrhœa is a common accompaniment, and, like the vomiting, easily produced by the most trivial circumstance;—yet, in these cases, the relaxation is usually the companion instead of the precursor of the severe attack, and partakes more of the nature of the evacuations in the second stage of the Epidemic Cholera. The absence, therefore, of the preliminary diarrhœa may perhaps be entirely ascribed to the poison being introduced into the system in smaller quantities than in the Epidemic Cholera; and to its conveyance onward, with the circulating current, before it has accumulated to a sufficient extent in the abdominal system of veins to produce a specific effect; for it is most rare, in the malignant ague, for the patient to fall into a state of asphyxia at the first or second accession. As, also, we find that climate or temperature exerts so great an influence in the production of the premonitory diarrhœa, we must infer that a certain degree of cold is necessary in order to determine the poison from the vessels of the exterior to those of the internal organs, in

quantity sufficient to produce a specific effect ; for, although the common, and, it may be added, the almost invariable precursor of the disease in temperate regions, it is as generally wanting in inter-tropical climates. Now as the malignant ague (which is alone to be compared to Cholera Asphyxia) is the complaint, not of a cold or temperate, but of a warm climate—and then only to be met with in the hottest season of the year,—the absence of the premonitory diarrhoea, in the above disease, is not more strange than an attack of Cholera, in India, which has not been preceded by this stage.

There is one other point which yet remains to be discussed before closing these remarks ; and this is, the physiology of the hot stage. It has been already cursorily observed, that I consider the consecutive fever of the Epidemic Cholera an effect of the same cause as that which produces the previous stages. That this is the case in intermittents there can be little doubt, inasmuch as we observe the *fever* last a certain and fixed period, at each accession, the same as the *cold* stage ; the patient remaining comparatively free from all ailment during the intervals. The theory, therefore, that would ascribe the consecutive fever of Cholera to the changes which take place in the relative composition of the blood ; and, as a consequence, to the loss of its serous portion, cannot be applicable to intermittent fevers, as the regular interval of repose, and alternations of morbid action, (independently of the absence of serous or other evacuations) are at

variance with such a conclusion. As, also, the consecutive fever of Cholera is not an invariable accompaniment of the previous collapse, being frequently wanting in the severest cases, we ought likewise to look, even in this disease, to another and a different cause. That cause (if the theory before given is a correct one, respecting the course of the poison in the system, and its situation in the different periods of the disease) would appear to be no other than the presence of a poison in the minute branches of the arterial system, or capillaries of the skin; by which the functions of these vessels become deranged, similarly to those of the lungs in the previous stage—but productive of a set of phenomena different from the foregoing in consequence of the difference in the offices which these capillaries perform in the animal economy.

But although I consider the physiology of the consecutive fever of Cholera, and of the fever stage of intermittents, to be the same, there is one difference which requires to be considered, in comparing these several diseases. I allude to the continuance of the fever in Cholera, until the death or convalescence of the patient; without being followed, as in intermittents by a second accession. This variation, however, only holds good with respect to the ordinary intermittent; as, in the malignant cases, with which alone we have a right to compare the Epidemic Cholera, the same phenomenon is witnessed as in the latter disease. It has been stated, when describing the state of asphyxia into which patients

fall, when labouring under the malignant ague, that the paroxysm is seldom followed by another attack; but ends either in the death, or recovery of the individual. It may now be added that, in such instances, the cold fit is not followed by the stage of reaction or of fever. In other cases, however—as I have myself had occasion to observe, and principally in those individuals in whom the stage of collapse has not been of the gravest type—the paroxysm is accompanied by the febrile stage, which, like that of the Cholera, lasts an indefinite instead of a definite portion of time; and becomes converted into a continued instead of an intermittent fever.

The difference here mentioned may be explained, in all probability, by a reference to the greater or less quantity of morbid matter in the one case than in the other; by which, when accumulated to a given extent, it becomes arrested in the minute vessels of the skin, the same as has been attempted to be proved takes place in the capillaries of the lungs, during the collapse of Cholera and the cold stage of ague.

That the poison of Cholera has some difficulty in passing through these vessels may be presumed, when we witness various eruptions on the surface of the body, during recoveries from severe attacks of that disease. This phenomenon I had an opportunity of witnessing more particularly in the hospital of St. Pablo, Barcelona, on my first visit to that establishment; and where I observed almost every patient that had recovered from the severe

attack of the disease, to be covered with a thick elevated eruption. In the other Cholera Hospital in the same town, only a few isolated cases of the same nature presented themselves, either then or at subsequent periods. This difference arose, no doubt, from the treatment adopted in the former hospital, viz.: bleeding, leeches in large numbers, and sudorifics, more particularly ipecacuanha; remedies which would naturally favour the course of the poison to the external surface, when productive of any good. It is, however, not a little singular, and by no means unimportant to remark, that in those cases subsequently treated by the carbonic acid (and there were many to whom this remedy was given in all periods of the disease, but principally in the second stage, to arrest the vomitings, and, in the commencement of collapse, when the remedies usually resorted to had failed), the same phenomenon was not witnessed. At least, I do not remember any case where the eruption appeared after the carbonic acid had been taken in its usual and proper quantity.

But we have a right to enquire, if the theory generally entertained respecting the physiology of fever is a correct one, how it happens that, with a more active circulation, and increased action in all the functions of the arterial system, the poison of Cholera remains for so long a period in the capillaries of the skin. To this I would reply, it yet remains to be proved that there is an increased circulation in these vessels, during the pyrexial stage

of any fever. On the contrary, I should be more inclined to believe, that the current or velocity of the blood, in the minute capillaries, is diminished; at the same time that the quantity is increased. As to the increase in the activity of the heart's action, and the pulsations in the larger arteries; this affords no reason why the circulation in the minute branches should not be, at the very same time, in a great measure suspended. In fact, the latter circumstance would seem to offer an explanation of the increased action of the heart and unnatural pulsation in the larger arteries—an effort on the part of this organ to overcome the resistance offered to the propulsion of the blood through the extreme branches of the arterial system. The sensation conveyed by the finger when applied to any of the larger arteries,—for it is to be remembered that we are unable to perceive the flow of blood by the same test in the minute capillaries—is, probably, altogether illusory, causing fulness of the vessel and distention of its coats, at each pulsation, to be mistaken for an increase in the velocity of the current which flows through its cavity. This is, in fact, the phenomenon usually witnessed in a common whitlow,—an example so often quoted to prove the muscularity of arteries. Without waiting to enquire whether arteries are endowed with muscularity or not, I would merely remark, that the effects witnessed in this affection, as well as all others attendant on inflammation, fever, &c., are at variance with the idea of increased action in, or, at least, increased

flow of blood through, the minute branches of the arterial system. But if it be allowed that the arteries are endowed with a contractile power, and that this contractility is dependent on nervous influence; we have then only to deprive the system of nerves, which presides over the distribution and circulation of the blood, of a portion of its vital energy, to account for the suffusion of the skin, distension of the minute arteries, redness, and other phenomena, witnessed in so many instances in the animal economy. By this hypothesis, and, as it appears to me, by this hypothesis alone, can we explain the action of those extraneous and poisonous substances which, when introduced into the system in large quantities, produce death by the annihilation of the nervous energy of the grand sympathetic; but which, when present in smaller quantities, cause only fever or inflammation. In one word, and not to expend more time on a subject which requires a separate consideration, I will state that I consider the phenomena attendant on inflammation and fever to be the effects, not of a state of excitement or increased action in the whole of the vessels of the arterial system, but of a cause which depresses the energy of those nerves which preside over the capillary system, by which the functions of these vessels become suspended, and the circulation in the affected part, partially, or entirely, arrested.

These cursory remarks seemed necessary, on my part, in order to meet any objection which might be offered to the conclusion before drawn:—

viz., that the consecutive fever of Cholera is an effect of the same cause as that which produces the previous stage; and, that to the presence of a poison in the arterial system, all the morbid phenomena then witnessed are to be referred. But if the foregoing points are conceded, we may then understand how it is, that the same poison, which, when present in the pulmonary organs, suspends their functions and entirely arrests the circulation of the blood, is also able, when carried forward into the capillaries of the skin, to produce the consecutive fever. It is an anomaly not to be explained, to suppose that the poison of Cholera should act as a narcotic on the filaments of the grand sympathetic distributed to the pulmonary organs; and yet, when introduced into the capillaries of the skin, cause general excitement and increased action—as we should be bound to believe, if it is allowed that the consecutive fever of Cholera is an effect of the same cause as that which produces the previous stages; and if the theory generally entertained, respecting the physiology of fever, is applied to the pyrexial stage of the above disease.

In closing these observations respecting the physiology of Cholera, it may be allowable to conclude that, as the effects witnessed are similar to those observed in the various forms of intermittent fever, the immediate cause by which they are produced in the animal economy is the same also. If this is conceded, we may then draw the following important conclusions:—

First: That the Epidemic Cholera is produced by the introduction of a poison into the system.

Secondly: That the poison in question existed previously in the atmosphere, and entered the lungs with the air inspired.

Thirdly: That the accumulation of the poison takes place in the venous system.

And Fourthly: That in the stage of collapse, the poison is situated in the capillaries of the lungs; and, in the consecutive fever, in those of the skin, as well as other portions of the extreme terminations of the arterial system.

TREATMENT.

HAVING endeavoured to prove that the disease known by the name of the Epidemic Cholera, Cholera Asphyxia, or Blue Cholera, is caused by the introduction of a poisonous substance into the system, the plan of treatment which, in this case, ought to be pursued, would appear to be that which is adopted with persons who have taken, either by accident or design, any particular or known poison. As, also, the poison of Cholera acts principally on the stomach and intestines, at least in the first stages of the disease, we may pursue the same course as when any substance, injurious to the health or safety of man, has been introduced into these organs by human agency. In these latter instances, the plan of treatment usually adopted has had two objects in view—the rendering inert or removing the poisonous substance out of the system, and the alleviation of those effects which may have resulted from the presence of the poison in the stomach or other organs.

This would seem to be the course which, both from reason and analogy, we ought to pursue with persons labouring under the effects of the Choleroid poison, as far as our means of induction and proof enable us to proceed. As, however, the poison productive of Cholera is of so subtle a nature, that

we have been as yet unable to discover or collect it, either in or out of the body, we are necessarily prevented from making any of those direct experiments so conclusive in other cases. We are unable to ascertain, by analysis, what the nature and composition of this poison is, or what are the substances capable of combining with it, altering its properties, or destroying its virulence. But although debarred from pursuing this direct and conclusive course, there are yet other satisfactory, though less certain, proofs to be obtained by experiments conducted within the body. We must, under these circumstances, endeavour to ascertain whether any substance which is administered, either with this view or any other, neutralizes the poison, the injurious action of which on the body is the cause of certain peculiar and specific effects. If any substance which is then given, and which exerts no sensible or general action in the economy, removes all the effects which had previously taken place, we have a right to infer, if the same result is obtained in a sufficient number of instances, that the remedy is an antidote to the poison. This conclusion, however, can only be drawn when no vomiting, purging, or other sensible or general effect, is produced after the exhibition of the remedy; for, otherwise, as Orfila has justly remarked, with respect to emetics given to persons who have swallowed any poisonous substance, we cannot be sure but that the restoration of the patient may depend on the expulsion of the poison, upon which the chemical re-agent has

not exerted any influence. Independent of the remedy which will hereafter come under consideration, I am not aware that any substance has been hitherto given which could be said to exert an antidotal power on the choleroïd poison, if we except the various combinations of alkalies. These substances have been administered, by different individuals, with the view of neutralizing any acid matter present in the stomach, under the supposition that acidity in the *primæ viæ* was the cause of all the morbid phenomena. But the result of the practice does not warrant this conclusion, while other facts also tend to negative such an hypothesis.

If unable to effect this object, the neutralization of the poison, our only resource, then, is to endeavour to remove it out of the system, by those means usually resorted to on other occasions, or such as experience points out as most eligible in this particular disease. Many various and different remedies, from their known action in the economy, and which have been employed by various practitioners, tend to produce such a result, and have doubtless effected it, when either of these methods has proved beneficial. The success which has attended the exhibition of calomel, a host of emetics, certain purgatives, and diaphoretics, can only be explained on this principle.

In deciding to which of these two plans of treatment we ought to give the preference, did not experience but too fatally teach us the general inefficacy of the latter, common sense would inform

us that the former must be the most certain, the most safe, and the only scientific plan of treatment, provided the antidote is a simple and innocuous substance. It will be my endeavour to prove, in these pages, that carbonic acid is that antidote; if so, nothing can be less hurtful or less injurious to the living frame than this medicine. It is not only the most simple and innocuous which can be employed, but one with which Nature has herself provided the animal economy, for effecting certain salutary purposes. As carbonic acid is secreted into the intestines, and always exists in the veins of a person in health, escaping afterwards by the lungs, its presence in these situations is, doubtless, for some wise and salutary object. Knowing that carbonic acid combines with, and renders innocuous, putrefactive and other substances injurious to animal life, it is neither unreasonable nor unscientific to conclude, that this gas neutralizes the effects of those noxious and excrementitious matters which always exist, to a greater or less extent, in such situations.

Having formed a particular theory respecting the Epidemic Cholera, based on the chemical researches of Dr. John Davy, who ascertained that the expired air of a patient labouring under this disease was deprived of its due portion of carbonic acid, I was induced to administer the different forms of carbon, for the purpose of confirming or refuting the truth of my doctrine. Although now bound to believe that the theory then formed was,

in many respects, an incorrect one, it led at least to a not unimportant conclusion—the conviction on my mind that both carbon and carbonic acid remedied the effects witnessed in the Epidemic Cholera, at the same time that they removed, by their specific action, the cause also. This will, it is hoped, be apparent to others by a recital of the results obtained from the exhibition of these remedies.

When carbonic acid has been given in any case in which the stomach is alone affected, the effect of the medicine, according to my experience, has been to relieve the symptoms almost immediately. The nausea is speedily dissipated, the giddiness and faintness disappear, and the sensation of burning and heat at the pit of the stomach is no longer felt, or complained of, after two or three doses of the medicine.

But the most remarkable circumstance is that which has attended the employment of the same agent during the stage of diarrhoea; of that relaxation of the bowels which is, almost always, the precursor of the severe form of Cholera. Of the numerous cases in which I have given the carbonic acid, at this period, it has invariably arrested, with some few exceptions to be explained hereafter, the morbid process; at a longer period, it is true, but still at a regular and certain interval.

Neither is the remedy less useful in what has been termed the evacuant, or second stage of the disease, characterized by rice-water evacuations. In these cases, the irritability of the stomach is

speedily relieved, and the vomiting ceases soon after the first, or at most the second, dose of the medicine—while the relaxation of the bowels is also arrested, if not with the same, with almost as great celerity as in the former instance.

But it is in the commencement of collapse that the efficacy of the remedy is best observed, and its *modus operandi* the most apparent. Not only is the vomiting immediately arrested, as in the previous stage; but the thirst, heat, and burning sensation at the pit of the stomach, disappear almost as speedily. During a repetition of the medicine, the spasms which generally prevail at this period are effectually relieved; the evacuations from the bowels become less abundant and less frequent; the depression of the system is removed; and the other symptoms characteristic of this stage vanish by degrees—so as frequently to leave the patient comparatively free from all ailment, after the administration of only five or six doses of the medicine.

This circumstance, which has frequently excited the attention and admiration of others, and has been dwelt on with peculiar emphasis by Drs. Ardevol and Pascual, in the reports which are added in the Appendix, admits of explanation by a reference to the theory which has now been broached respecting the physiology of this disease. As the same effect is witnessed by the administration of the remedy in the cold stage of ague, and as the same arguments which have been used in the latter instance are applicable also to the former, it is only

now necessary to call attention to this not unimportant phenomenon.

Putting aside the cessation of the vomiting, which may be referred to a local action of the remedy, to what, I would ask, are we to ascribe the relief of the other symptoms? To what are we to refer the cessation of the purging? Not to any astringent property of the remedy, as I have often ordered this medicine with success in cases of constipation; thus showing that, if it exerts any action on the large intestines, it is that of a relaxant, rather than that of an astringent. Had the pure carbon been alone given, the conclusion might have been different, inasmuch as we know that this agent, or the substances with which it is combined, possesses properties of an astringent nature. As, also, the common saline effervescing draught has usually been administered, tartrate of potash or soda, a purgative medicine, has been taken at the same time. True it is, that the quantity thus taken would not have been sufficient, in every case, to produce a purgative effect with persons in health; yet as, at the epidemic periods alluded to, even an excess of ripe fruit, or vegetables, is so generally followed by relaxation, it may be asked how it happens that the contrary result is obtained in this instance.

Again—to what are we to ascribe the relief of the spasm? Not to the anti-spasmodic property of the medicine, for this remedy has no action in common with the greater number of those belonging to the class of antispasmodics.

And, lastly ; to what is the removal of the depression of the nervous system and the state of collapse owing?—to the stimulant property of the remedy? Assuredly not ; because it possesses, if any action, that of a sedative, as is well known when this gas is inspired into the lungs in any quantity. Neither can we ascribe the removal of the latter symptoms to the relief afforded to any of the others ; inasmuch as these various phenomena can only be common effects of one common cause, being sometimes present, sometimes absent. Besides, I have experienced the same results when vomiting, or purging, or spasms, or collapse, were alone present, and all the other symptoms common to the disease altogether absent.

How, then, we may ask, can a remedy act which arrests the most severe and long-continued vomitings at the first or second dose, and this, too, after every other calmant and narcotic has failed ;—which, possessing no astringent property, puts a stop to the most profuse purging ;—which, endued with no known anti-spasmodic virtues, instantly relieves the severest spasms ;—and which, although acting as a slight sedative, produces re-action, and removes the depression of the nervous system at a time when some of the most important of the vital functions are almost entirely suspended? A remedy which produces such opposite and different effects, and which has no sensible or direct action in the economy, can only act in one way—that is, by removing the cause of these various phenomena ; and

as that cause has been proved to be the presence of a poison in the system, the remedy which removes it must neutralize the poison, and must be, consequently, an antidote in the disease.*

We know not what the nature of this poison is, or whether it exists in a solid or a gaseous state ;

* In stating my belief that carbonic acid is an antidote to the poison of Cholera, and that, when introduced into the system, it is capable of neutralizing the morbid matter productive of the Epidemic Cholera, I presume that no objection will, or can, be offered to such an inference. All doubts on this point would appear to be solved by the result of the experiments of Drs. Lawrence and Coates, as detailed by Dr. Copland in his Notes to Richardson's Physiology. In reasoning upon the subject of absorption, the question has frequently arisen (observes this writer), whether the articles found in the living fluids exist there as chemical substances, or have their chemical nature altered and annihilated by the action of the vessels through which they have entered the system. It was, therefore, deemed a curious subject of inquiry, whether artificial chemical changes can take place in the fluids while they continue to circulate in living vessels, and the ordinary actions of life go on. With a view of ascertaining this point, they commenced by throwing prussiate of potash into the cellular substance, and green sulphate of iron into the abdomen, in order to try whether the well-known result of their admixture, prussian blue, would be produced in the vessels. On performing this, they were gratified by the striking result of a distinct and beautiful blue in the thoracic trunk and its contents, and in nearly the whole substance and surface of the lungs.

It is also an important coincidence, as regards the theory now broached respecting the supposed union of the poison and antidote in the centre of the circulating system, that when the above order was reversed, and the sulphate of iron injected into the cellular tissue, as the substance of more difficult absorption, and the prussiate into the abdomen, where the process goes on with more facility, the chemical combination witnessed in the former instance did not take place, as no prussian blue could be discovered in the body.

and can, therefore, only speculate on the changes which may take place, or the combinations which may be formed when brought into contact with other substances. But judging from the effects of the remedy, we may be permitted to conclude, that it is a gaseous and not a solid substance, both carbon and carbonic acid being familiar to us as chemical agents for combining with, and neutralizing, the gaseous products of putrid and other poisonous matters,—and hence termed antiseptics. Carbon possesses the remarkable and singular property of absorbing several volumes of its own bulk of gas; and it has been proved, that even water has been formed within the pores of recently prepared charcoal, by the absorption and condensation of the gaseous substances of which this element is composed. Charcoal possesses this property to the greatest extent when quite fresh, losing it entirely after it has been kept a given time; so that its absorbing power is in an inverse ratio with its age;—a fact of some importance in a practical point of view, as it will be afterwards shown, that the good effects derived from the use of this remedy are only witnessed when the charcoal has been recently prepared, and preserved from the contact of the external air. The union of carbonic acid with the same matter is probably effected by that mutual attraction which gases, even of a different nature, have for each other; and by which a gas confined within a common bladder escapes through its coats,

and is replaced by the air surrounding it, until a mutual absorption and equal diffusion of the two elements have taken place.

But whatever explanation may be offered on this subject, one thing at least is certain,—that many poisonous substances, more particularly septic ones, have their injurious properties destroyed or rendered inert when combined either with carbon or carbonic acid. Knowing, therefore, the properties of the various forms of carbon, and observing that, when taken by individuals during an attack of Cholera, they instantly arrest the course of the disease, we can only conclude that these agents, when thus given, combine with and neutralize the poisonous matter productive of the effects witnessed in the Epidemic Cholera.

It now only remains to ascertain the influence which the remedy exerts in the state of confirmed collapse. When it has not been previously administered, its employment at this period is not constantly or generally followed by any sensible effect, or beneficial result. This, however, is only what might and what ought to have been expected from the employment of such a remedy at such a period, there being several causes in operation, either of which alone would, it is probable, satisfactorily account for the failure observed in this instance. These causes may be conveniently arranged under three distinct and respective heads, viz. :—

The stagnation of the circulation ;

The situation of the poison ; and

The depression of the nervous energy.

In the first and second stages of this disease, and, generally speaking, in the commencement of collapse also, we should, *à priori*, have been led to infer that no obstacle exists to the complete neutralization of the poison ;—an inference confirmed by an extended experience in the administration of the remedy now under consideration. But in the state of confirmed collapse, when the circulation is entirely arrested, the same result can hardly be expected to follow, at least by the introduction of the remedy into the stomach. It is then more than doubtful if absorption can take place, excepting to a very limited extent, as the experiments of Magendie on animals have clearly and satisfactorily demonstrated.

But, in addition to the above cause, if any faith is to be placed in the theory now broached ; or, if we are allowed to draw analogical deductions from the result of the treatment, by the same remedy, in the cold stage of ague ; we must also conclude that there is another obstacle to the complete neutralization of the poison at this period. I allude to the presence of the noxious matter in the capillaries of the lungs, a situation to which the antidote can only arrive in part, under common and ordinary circumstances. Whether the method about to be pointed out for the employment of the antidote in the state of confirmed collapse, will be attended with

greater success than has hitherto been the case, yet remains to be ascertained.

But, although we were certain of accomplishing this most desirable object, the neutralization of the poison, it is doubtful whether we should, without the assistance of other adjuvants, be also able to remove the state of collapse, when it has become confirmed, or has been of long continuance. It is clear that, at this particular period, two things are necessary,—first, to remove the cause, and then to remedy the effects of that cause; both which are probably to be effected by opposite and different means. It is not likely that the agent which neutralizes the poison, (the injurious nature of which on the system is the cause of the collapse,) will also be able to remove the collapse itself, unless it were a compound substance, possessing, besides its specific properties, those of a stimulating nature, or such as act specifically on the nervous system. In cases of partial collapse, the mere removal or neutralization of the poison has been of itself sufficient to restore the patient, as the history of numerous cases tends to prove. But in the majority of instances of confirmed and long-continued collapse, can the same effects be expected to follow similar or other more specific plans of treatment? Experience obliges us to declare that such a result has been but seldom witnessed as yet, and that it will be but seldom observed in future, if the disease is allowed to run on unchecked to the state of confirmed collapse. When this state has existed for a certain length of

time ; when the blood has ceased to circulate in the body, perhaps for hours ; when every vital function has become suspended, and the energy of the nervous system all but annihilated,—the organic life of the individual may then be said to have become extinct, and that portion of the system as lifeless as the body which has ceased to breathe. If such is the state of a patient in the collapsed stage of Cholera, can it be expected that the mere removal of the poison, were we able to accomplish this, will be sufficient also to remedy the effects of that poison on the system ? Reason and analogy both lead us to answer in the negative. When a man has received a severe blow on the head, by which a portion of his skull has become fractured and depressed, the pressure of which on the brain has been sufficient to deprive him of consciousness and voluntary motion ;—if the skull is trephined, and the depressed portion elevated, soon after the accident, the mere elevation of the bone will be adequate to the restoration of the patient. But if this pressure has been continued beyond a certain time, the energy of the brain will have become so far destroyed, that the application of the trephine, and the use of the elevator, will not be sufficient to restore him to consciousness or voluntary power ; and he sinks, the victim of delay. So, again, in all the various and different classes of poisons, when any of these have been taken, either by accident or design, if they are immediately neutralized or evacuated from the stomach or system, the effects resulting from their exhibition also dis-

appear, with the neutralization or removal of the poison. But, if a particular interval has elapsed before these desirable objects are effected, we shall then have to remedy the effects which have resulted from the longer presence of the poison in the system; effects but too frequently followed by the extinction of life.

That in the collapse of cholera the nervous energy is in a state of great depression, there can be little doubt; inasmuch as the suspension of those functions over which the grand sympathetic presides, must be in a great measure ascribed to the deleterious influence of the poison on these particular nerves. Although differing with some writers, who wish to ascribe the state of collapse entirely to the loss of nervous energy in the ganglial class of nerves, and believing that the arrest of the circulation is in part due to local as well as general causes, it is nevertheless certain that, when this state has existed for a certain period, the vitality of these nerves becomes almost entirely destroyed. If, therefore, carbonic acid is an antidote to the poison productive of the Epidemic Cholera, it is evident that, in the state of confirmed collapse, another and a different agent is required to excite the nervous system; as the above remedy, if it exerts any direct action in the economy, acts rather as a sedative than a stimulant.

But although it is necessary to resort to these adjuvants for the removal of the collapse, we must not suppose that the latter class of remedies is alone

necessary at such a time ; or that their exhibition would be attended with general good results, unless preceded or accompanied by some antidote, or other agent, capable of expelling the morbid matter out of the system. As the stagnation of the circulation, and the suspension of those functions termed vital or organic, are produced by the presence of a poison in the animal economy, the state of asphyxia can only be an effect of a particular and antecedent cause. The death of the individual also is due, not to the effect, but to the cause ; for the extinction of life is but an effect, or the sum total of all the effects produced by the same cause. How, then, could we hope to save life, if our views were limited to the remedying an effect, the state of collapse, without having any regard to the removal of the cause, the presence of a specific poison ? To attempt to arouse the nervous energy, at a time when another agent is in operation which tends to depress it, without taking any means to rid the system of its noxious presence, would be like pouring oil, as well as water, on the fire which we were endeavouring to extinguish. But, even although we should succeed in exciting the dormant energy of the nerves and restoring the circulation, the patient would again fall into the same state, if the means resorted to were not such as to favour, or to cause the expulsion of the poison out of the system. As, however, this proposition is so self-evident, I shall not attempt to pursue the subject any further, but merely add, that the proper and only scientific

plan to adopt, under these circumstances, would be, before resorting to stimulants, to administer such remedies as are capable of expelling the poison out of the system ; or, what is better, render it inert, by the introduction of some other agent which has an affinity for it.

In conclusion, I will only add, that we shall never arrive at a fair and impartial decision respecting the antidotal power of any remedy in this disease, if we limit our view of its operation to that period when the collapse of the system is complete. We can never then judge whether the poison has become neutralized or not ; as the mere neutralization of the poison, alone, may not be sufficient to save the life of the individual. In using other means and other remedies, as we necessarily must, to combat symptoms, the effects of the poison on the system, we run the risk of mistaking the action of these remedies, and confounding those which tend to remove the cause with those which are only able to remedy the effects. We are consequently necessitated to restrict our view, in seeking for an antidote, to the first periods of the known operation of the poison on the system ; or, in other words, the first two stages of the disease, and the commencement of collapse. Having satisfied ourselves of the efficacy of any antidote, in these stages, let us not only trust to it during the same periods in other cases, but even in that of confirmed collapse — in combination, however, with other

remedies, which act as excitants on the nervous system.

Should we fail, we shall have the satisfaction to know, that our want of success is due, not to the wrong measures adopted, but to the insufficiency of the means for the end desired ; and we shall learn that, in this disease, as in all others, there is a limit to human skill, and a power which presides over life and death greater than that possessed by weak and finite man.

DIRECTIONS FOR THE ADMINISTRATION OF THE REMEDY.

At the first onset of the attack, when those symptoms are present which denote derangement in the stomach, unaccompanied or preceded by diarrhœa, any of the preparations which contain carbonic acid may be given and repeated every hour until all unpleasant symptoms are entirely dissipated.* The first dose has always, with me, given immediate relief, and the third, at most, removed every symptom but that of a peculiar sensation of lassitude and languor.

* As it is absolutely necessary that the medicine should be taken in a proper manner, for otherwise the patient will only be swallowing a simple solution of tartrate or citrate of soda, instead of a certain portion of carbonic acid, it may not be superfluous to point out what I consider the best mode of preparing the remedy.

Thirty grains of the powdered carbonate or bicarbonate (not the sub-carbonate) of soda, or potash, should be placed in a large tumbler, to which is to be added a dessert-spoonful of any simple syrup, mixing the two ingredients together so as to form a homogeneous mass. Then take twenty grains of citric or tartaric acid, and dissolve it in a wine glass full of water, when the solution is to be poured in the contents of the tumbler, and the mixture drank off immediately, before the effervescence has subsided. If more convenient, or when to be allowed, lemon-juice may be substituted for the citric or tartaric acid, in the proportion of two table-spoonful of lemon-juice to the same quantity of soda or potash. As the object, in giving the syrup, is to render the mixture more tenacious, and prevent the gas escaping so rapidly as would otherwise be the case, it is not necessary to use any, or at least, so great a quantity in the latter as in the former instance.

In the preliminary diarrhœa, three or four doses of carbonic acid is, in general, sufficient either to arrest the diarrhœa, or change the character and appearance of the evacuations. Should the relaxation continue, it will be advisable to suspend the administration of the gas, and substitute the pure carbon or prepared chalk, either of which preparations ought to be continued until the purging has entirely ceased. This difference in the effects of the remedy at these two periods may, perhaps, be ascribed to the circumstance that, in the latter instances, the carbonic acid does not reach the large intestines in sufficient quantity to arrest the morbid process, the principal part of the gas being absorbed from the stomach and conveyed onwards to the lungs, where it will escape with the expired air. The greater benefit derived from the chalk may, therefore, depend on the carbonate not being decomposed entirely in the stomach, nor the whole of the gas liberated until the remedy reaches the large intestines. The astringency of the lime will also tend, at this period, to effect the object which we have in view; for although I have but too often witnessed the inefficacy of pure and simple astringents, when given alone, they may be advantageously employed when only intended to remove the simple relaxation consequent upon the previously depressing operation of the poison; that is to say, remedy an effect, the cause which produced it having been before removed by other and different means. The superior efficacy of the pure

carbon may also be explained on similar grounds—the non-decomposition of this substance in the stomach, and its arrival in the large intestines in the same, or nearly the same, state as when first introduced into the above organ.*

But it is not alone sufficient to remove the symptoms which may have existed in the above divisions of the first stage of the disease, it will also be necessary to continue the administration of the gas, at longer intervals, until we suppose that all danger of the supervention of the other periods has entirely passed. Although the theory which has now been broached offers an explanation why this course should always be adopted,—inasmuch as it shows that the antidote which is administered in the first stage of the disease will not, in all probability, come in contact with that portion of the poison which arrives, at a later and subsequent period, in the pulmonic organs, from the veins of the extremities and superficies of the body,—I would not rest my practice on theory alone, did not experience point out the necessity there is for following

* In making use of this remedial agent, it is of the utmost importance that it should be recently prepared, otherwise little or no benefit will attend its exhibition. In fact, it ought to be prepared at the moment, or, at least, for the occasion, when the disease is prevailing epidemically. The most simple method of obtaining it, and one that will answer all intended purposes in cases of emergency, is to burn a piece of cork until it becomes quite black and carbonized, when it should be powdered and given in a little milk and water, or water alone.

the above rule. This will clearly appear by the perusal of case 6, in which it is evident that the administration of the gas in sufficient quantity to remove all the symptoms present in the previous stage, did not, nevertheless, prevent the subsequent appearance of slight symptoms of collapse. Having witnessed the same occurrence in numerous and repeated instances, where the medicine has been suspended after its administration, in the first stages of the disease—the intensity of the subsequent attack varying, however, in almost every case, seldom being very severe, and sometimes so slight as to be scarcely perceptible—I now always make it a rule to advise the continuation of the remedy until it appears that all danger has entirely passed by. When this period is, will, of course, vary with different individuals; but it cannot be difficult for a professional man, conversant with the *reigning type* of the epidemic, and acquainted with the previous march of the disease in that particular case, to form a fair criterion of the probable time when the collapse would have supervened, had the disease run on unchecked to that stage. As, also, no harm can result from the employment of the remedy for weeks, much less days, (administered every three or four hours) it will always be allowable to err on the right side, and give the patient the benefit of any doubt that may arise on this point, by continuing the medicine for a longer period than may be considered absolutely necessary for his safety or recovery.

In the second stage of the disease, characterized by rice-water evacuations, as the malady has then arrived at a point from which it always proceeds, at an accelerated pace to the next period, it will be requisite to administer the remedy at shorter intervals, as every half hour, until not only the vomiting, but the purging also, is entirely arrested.* If the first dose, as sometimes happens, is rejected, it should be immediately repeated without waiting for the regular interval. So, again, after four or five doses of the carbonic acid have been taken, if the purging continues to any extent, it will be advisable to administer the pure carbon, either by the mouth or by injection; giving a table spoonful every hour in the former case, or three or four table spoonsful in each enema, which the patient should endeavour to retain as long as possible. Or, instead of the latter, if we wish a more speedy and certain result, we may inject a solution of soda and tartaric acid, either in a state of effervescence, or, what is better, in succession, so as for the disengagement of gas to take place in the intestines.

* Instead of the saline effervescing draught before recommended, soda or seltzer water may also be administered. One objection, however, which applies to these, and all other kinds of bottled aerated liquors, is, that during moments of anxiety and sickness, unless the patient and the attendant are well accustomed to the administration of such drinks, it is seldom that the draught is taken before a considerable part, or nearly the whole, of the confined air has escaped from the containing fluid. In this case the remainder would be, in a great measure, useless; so that, under pressure, water absorbs and retains but a small quantity of carbonic acid gas.

Collapse.—In the commencement of the stage of collapse, if no carbonic acid has been previously administered, a draught should be given every quarter of an hour, until three or four doses have been taken. After this, the medicine may be repeated at longer intervals, as every hour, while the purging continues, and every two hours when the evacuations have become entirely suspended. When the purging is severe, or of long continuance, it is always necessary to administer the pure carbon, both in the form of injection and by the mouth, in addition to the carbonic acid.

But if, after this course has been adopted, no relief appears to be produced, it will then be advisable to resort to some stimulant; and the best that can be used at this period is, in my opinion, the carbonate of ammonia, which may either be given alone, or combined with an acid in a state of effervescence, according to the number of doses that have been taken of the soda, and the necessity there may appear to be for the repetition of the gas. On this point no general rules can be given, as much will depend on the state of the patient, the effect that has been produced, and, lastly, the previous state of the bowels—as, when the evacuations are profuse and frequent, we have a right to presume that a great part of the remedy escapes out of the body by this channel without being absorbed.

If the above means fail, or should we be called, for the first time, to a patient in a state of confirmed collapse, we shall then be obliged to resort to other

and different measures. Concluding that the principal part of the poison productive of this disease is situated in the pulmonary organs, at this stage of the disease, and inferring that it is impossible to bring the antidote in contact with the whole of the morbid matter, by its introduction into the stomach, it becomes a fair question to inquire whether there is not another and more direct way of accomplishing this object. I allude to the inspiration of the gas into the lungs. This is a measure which I have long had in contemplation, having prepared an apparatus for the purpose so long back as the autumn of 1832; but the general opinion entertained respecting the operation of this agent, when introduced into the arterial system, prevented my carrying this intention into effect. My principal reason for delaying this trial was the consideration that, as the pulse was wanting in those cases in which alone it would be requisite to resort to this method, we had no guide by which to judge of the effect it might produce on the system. Added to this, I was doubtful whether the state of collapse was due to the general depression of the nervous energy of the grand sympathetic; to the action of the poison on the pulmonary organs; or to its situation in the cavities of the right or left side of the heart. My subsequent investigations having convinced me that the poison is situated in the capillaries of the lungs at this period, and that the state of collapse is due to local as well as general causes, it is not unreasonable to infer that, if we

can bring the antidote in contact with the poison in this situation, its consequent neutralization, by removing the cause of the collapse, may either be sufficient to remedy the effects also, or else render other adjuvants more prompt and certain in their operation. Having since then made various trials by the inspiration of carbonic acid gas with persons in health, and finding that, when diluted with a certain portion of atmospheric air, no ill effects are produced in consequence, I should now have no hesitation in administering the same remedy, in the same way, in cases of disease also. In addition to this, having also introduced this medicinal agent into the lungs in a disease which bears so great a similitude to the Epidemic Cholera, more particularly in that stage where the only difference would appear to be a difference of degree, and having obtained favourable results from the operation, it will not, I trust, be considered empiricism on my part to propose a trial of the remedy in the way here pointed out. When diluted with two or more parts of atmospheric air, no harm can, I conceive, arise from the operation, as it is more than probable that the injurious and mortal effects which arise from exposure to the fumes of charcoal, are due rather to indirect than direct causes; that is to say, that they are produced by the substitution of carbonic acid gas for the oxygen of the air, without which animal life cannot be supported but for the shortest possible period. As, also, carbonic acid cannot be inhaled into the lungs in a state of purity, the

glottis being immediately closed by a spasmodic contraction of the muscles, a person who dies in an atmosphere of this gas may be said to perish, not from the injurious properties of the above agent on the nervous system, but from simple suffocation. But in this disease there is another circumstance, which diminishes any thing like what may be considered danger by the inspiration of carbonic acid gas into the lungs. This is, the stagnation of the mass of blood, and the arrest of the circulation in those vessels into which this chemical agent has to be introduced. In this case, the whole, or the greater part of the gas will, in all probability, again pass out of the body by the same channel as that by which it entered, unless from some cause with which we are at present unacquainted a certain portion is retained in the capillaries of the lungs.

Should this be proved to be the case, we have yet a resource for remedying the above effect, and which, it is probable, may hereafter be found not only an agent in removing the antidote from this situation, but a powerful adjuvant in restoring the suspended circulation. The measure that I would propose for the purpose of effecting this object, is the inspiration of heated air—an operation to which many valuable advantages would seem to be attached. In the first place, should the carbonic acid which has been inspired, accumulate in the lungs, or should its presence appear to produce any unpleasant symptom, the introduction of air into the trachea, in a state of rarefaction, would be the most

likely, if not a certain, means of elevating this gas from the situation in which it is confined.

On the other hand, if, as we have reason to believe, the morbid agent in this disease is an aerial poison, the adoption of the above measure would not only have the effect of elevating the carbonic acid gas that may be contained in the pulmonic vessels, but also tend to extract any portion of this poison that may remain unneutralized, through the permeable coats of the containing arteries. That this gas, or poison, obeys the laws to which other gaseous substances are subject, we have a right to conclude ; while also it is more than probable, that to this property alone is to be ascribed that difference which the disease presents in opposite climates—for instance, India and England. In the former country, the type of the disease was that of sudden and complete collapse—unaccompanied, in most instances, with intestinal evacuations ; thus showing that, from local causes, as temperature, &c., the poison was propelled by some powerful attraction, (as water is raised in a pump by a force resembling suction) from the venous extremities to the capillaries of the lungs ; or what may be termed the extreme termination of the venous trunks. But as the poison of cholera appears to traverse the minute capillaries with great difficulty, and seems only capable of being expelled from these vessels by the most active means, we find that the same cause which tended to attract this poison to the lungs was yet insufficient, in the majority of instances, to draw

it through the transparent and permeable coats of the containing vessels. In other instances, however, when aided and assisted by the efforts of art, this result was certainly attained ; for not only were the recoveries, in the stage of collapse, greater in proportion in India than what has since been witnessed in Europe—but, in such cases, the consecutive fever was, with a few solitary exceptions, invariably wanting ; thus showing, that the poison was ejected out of the body from this situation, instead of being propelled into the arterial system.

In recommending this adjuvant, I take it for granted that it will be resorted to only with those limitations, and under those restrictions, which are necessary in the employment of an agent which may be useful or injurious, according to the time or manner in which it is employed. Thus, if resorted to in cases of sudden collapse unaccompanied by evacuations from the stomach and bowels, and before any means have been used to neutralize or remove the poison supposed to be accumulated in the venous system, the inspiration of a more rarefied atmosphere might only have the effect of attracting a greater portion of the poison from the extremities to the centre—thereby making the collapse more intense than before. Believing that it would take a longer period, and a more rarefied atmosphere, to extract the poison through the coats of the containing vessels, than to raise it, in combination with that fluid in which it is suspended, from one situation to another, we should resort to such an

adjuvant with caution at that particular period when the deleterious agent is being propelled, by the action of other and different causes, from the extremities to the centre. The plan which ought to be adopted, in these cases, would be, if the collapse has not been preceded by previous evacuations, to extract blood from the arm, to relieve the plethoric state of the venous system; and, then, to resort to those means already pointed out, for the neutralization of the poison; or such others as the practitioner may deem most convenient and suitable for its expulsion, before exposing the patient to a heated or rarefied atmosphere.

If, unfortunately, the above measures are found insufficient to produce the effect desired, we may advantageously combine another and a different adjuvant, that of a stimulating diaphoretic. I can recommend such a medicine with the more confidence, from the good effects I have witnessed after the exhibition of what are termed the powders of *vivorera*, extensively employed during the prevalence of the epidemic in particular parts of Spain. These powders, which are composed of equal parts of "*Eryngium campestre*, (Lin.) *Echuun vulgare*, (Lin.) *Alisum spinosum*, (Lin.) and *Nepeta marifolia*, (Lin.)" were originally prepared from an old receipt which fell into the hands of D. José Melgarejo, Pharmaceutist of Murcia; since which they have been employed, as a specific, in that part of Spain, in cases of poisoning from venomous reptiles and insects, and also in hydrophobia. Of their

virtue in these affections no doubt can exist; but that their efficacy is due to general, and not specific causes, seems also equally certain, inasmuch as they appear to have produced the like good results in cases of mineral and vegetable poisoning. As their action in the economy is that of a stimulating diaphoretic, to this effect must be ascribed all the cures that have been obtained by this remedy; as we cannot suppose that the same combination would neutralize the poison of reptiles, or other animals, and, at the same time, destroy the irritant and corrosive properties of cantharides, arsenic, and corrosive sublimate. The same remarks will apply to the Epidemic Cholera; for although they were proposed and employed as specifics in this disease, I have only to add that, in one hospital, in which they were almost entirely trusted to in the commencement of the epidemic, they were subsequently abandoned as useless, and their place supplied by carbonic acid. This is a fate that must attend all remedies which act only on general principles, when trusted to singly, and uncombined, in every stage of a disease which presents various and different effects at each separate period. If I am not mistaken, the good effects resulting from the administration of this remedy are chiefly limited to the stage of collapse, as we should, *à priori*, have supposed from a consideration of the *modus operandi* of the medicine. It is then that a stimulant, by exciting the nervous energy, and a diaphoretic by relaxing the minute capillaries, and favouring the escape of the

pent-up matter, may possibly produce a favourable result. This effect is more likely to be witnessed when we have, by the previous administration of an antidote, succeeded in neutralizing a portion of the poison productive of all the dangerous symptoms, without having been able to effect the neutralization of the whole; or remove the depression produced by the previous action of the morbid agent on the nervous system. It has been under these latter circumstances that I have had reason to form a favorable opinion of this medicine, and I venture to recommend the same, or others whose action in the economy is similar, to the consideration of my professional brethren, believing that they may be advantageously resorted to for fulfilling the above indication.

Although past experience proves to me that it is highly injurious to resort to stimulants, before we have taken proper means to rid the system of the noxious presence of the poisonous matter, the cause of all the dangerous symptoms; yet as we are, at times, in want of some excitant to rouse the latent energy of the nervous system, I would wish, in this place, to call attention to one remedy which has proved, in my hands, a powerful adjuvant in cases of long continued or confirmed collapse. This remedy is the sulphate of zinc. From rather an extensive employment of this medicine for several years, I am induced to conclude that it is not only a tonic and an astringent, but, in large doses, a powerful excitant of the nervous system; that is to

say, of the ganglial class of nerves, upon which its chief energy seems to be exerted. The cases best adapted for the employment of this remedy, according to my experience, are those in which, from peculiar idiosyncracies, previous disease, or other depressing causes, the nervous energy is, at the time, in a state of great depression; and to remove which a powerful excitant is required, even after the poison has become neutralized or expelled. But in order to produce this effect, it is necessary to administer the remedy in quantities that would be considered unusually large under any other circumstances. In common affections I have given it in doses of from one to five grains, the latter being generally sufficient to produce slight nausea, if not vomiting, when the energy of the stomach is not below its ordinary or healthy state. But, in this disease, I have frequently given twenty grains, and repeated the same quantity several times without producing any vomiting.

In submitting this medicine to the attention and consideration of the profession, I think it most advisable not to give any directions for its administration, as every one will be able to judge in what manner, and in what doses, it ought to be taken in each individual case. Besides this, it must be difficult, nay almost impossible, to give any general rules for particular cases, as the difference of constitution in different individuals, the energy of the nervous system, and the greater or less intensity of the attack, will require that the remedy should be

taken in different doses by almost every individual patient. I would, however, remark that I think it most advisable to give it in that quantity which shall produce slight nausea, as otherwise we are not sure that the nervous system feels the impression of this agent. But when this object has been accomplished, the subsequent doses ought to be diminished in quantity, as it is not desirable to produce this effect on any other account. As to the time necessary for the continuance of the remedy, and the intervals at which it ought to be taken, we must be guided, not only by the violence of the attack and the danger of the patient, but also by the state of the circulation, and the impressibility of the system, generally, to the action of the medicine. In certain individuals, who, either from constitutional peculiarity, illness, or any other cause, are in a state of weakness and debility, but, more particularly, of nervous depression, it will be necessary to resort to that or some other adjuvant to restore the lost vitality of the body, not only in greater quantities but also at an earlier period of the attack.—As with individuals, so also with nations—the inhabitants of warm and tropical climates, but more particularly malarious ones, (whose bodily strength is always less, and their nervous system more, easily depressed than the natives of cool and temperate regions,) may require this excitant to be given more freely, and at an earlier period of the attack.

Having recommended this adjuvant in the

treatise published by me in Spain, it was resorted to at Cadiz in the case of a young Spanish physician, who, although he had passed with immunity through the severe eruption in Paris, while pursuing his studies there, was attacked with the disease shortly after his arrival in the above town. This patient, who was at first treated on the plan entirely antiphlogistic, or I should say, rather, according to the method of Broussais, very soon fell into a state of complete collapse. He had been in this condition about twelve hours, and his case was given up as hopeless, when the attendant physician was prevailed upon by my young medical friend, Mr. Crawford, who had read my manuscript only the day before, to try the plan of treatment recommended therein. This being agreed to, the carbonic acid was given two or three times first, and then the administration of the sulphate of zinc was commenced, in combination with the aristolochia, a stimulating diaphoretic much resorted to in the south of Spain, in the treatment of this disease. Ten grains were given every two hours, and, in the intervals, an effervescing draught, which sufficed to restore the patient, after the administration of, I think, 120 grains of the zinc; for, having mislaid the notes given me of this case, I quote now only from memory.

But we must be prepared for cases in which all these measures fail to produce the desired end; or in which we are called, for the first time, to a patient in a state of confirmed collapse, whose

situation is so dangerous, and the period allotted for the efforts of art so short, that nothing but the most active and vigorous treatment will be sufficient to raise the sinking powers, or keep alive the flickering flame. Our only resource, under these circumstances, will be to trust to those stimulants which are known to act with the greatest energy on the nervous system—not failing, however, to resort to the antidote itself in some one of its forms or modes of administration, in all those cases in which it has not been previously given. The most certain course to adopt, in such instances, would be, if the theory which has been maintained in this work is a correct one, to apply a stimulus directly to that portion of the system where the poison is supposed to be situated, instead of acting indirectly on the part affected by stimulating other and distant organs. This object would be, in all probability, best attained by introducing a stimulating gas into the pulmonary organs, or exciting the energy of the thoracic nerves by galvanic shocks. Fortunately we have some practical facts in proof of the possible efficacy of these measures. The late Mr. Finlayson, during the prevalence of this disease in Ceylon, tried the effects of the disengagement of ammoniacal gas, so as to impregnate the air inspired by the patient; and in two, out of three cases, in which the experiment was made, it appeared to restore the lost functions of the lungs. In another individual, a galvanic current, passed through the chest, succeeded in restoring the patient, although

previously nearly "moribund." These results, while they tend to confirm the theory already broached, hold out, at the same time, encouraging hopes of what may possibly be accomplished, even for those cases hitherto considered almost beyond the efforts of art, and whose state has been aptly compared to that of a "living corpse."

These are the principal, if not the only, measures which I consider it advisable to adopt for the cure of this disease. There are some others, however, which would seem to require a short consideration.

And first, as to Frictions. If we consult only experience, and set aside all preconceived opinions and theories on the subject, we shall be forced to confess that this operation is, to say the least of it, useless; and not very unlike the attempt to wash the black man white. This, however, is not all; for it appears to me that the resorting to this process, in the early part of the stage of collapse, has often been productive of much injury, by exhausting the energy of the system, and diminishing, instead of increasing, the subsequent temperature of the extremities and external surface. Considering that the generation of animal heat is all but impossible at that period, when the functions of the lungs are entirely suspended, nothing should be done that can possibly tend to exhaust prematurely the small quantity of caloric that may yet be retained in the nearly inanimate body. Such a result must certainly be expected to follow the application of

strong liniments and spirituous embrocations, perseveringly used by three or four assistants.

So, again, as regards the value of the application of external heat by means of the vapour and hot air bath. With respect to the former, the trials that have been given to this agent in India, in Russia, and lastly in England, not having answered the expectations that had been formed respecting its utility, it seems now to have been abandoned by the generality of the profession. Not so, however, as regards the hot air bath, for this method of applying heat has not only met with more advocates, but seems to have been frequently attended with favourable results. As, however, I believe this to have only occurred through sympathy, the relaxation of the capillaries of the skin having caused a similar effect in those of the lungs, I should henceforth prefer the direct method before pointed out to an adjuvant which acts only on the affected organs, through their sympathetic connexion with another and a distant part of the body. Independent of this, in numerous cases, notwithstanding the coldness of the external surface, the application of heat is highly distressing and seemingly injurious. The bedclothes themselves are often oppressive, and it is with difficulty the patient is persuaded to retain even a sheet, so anxious is he to expose his body to the contact of the external air.

On these several accounts, therefore, we must consider even the hot air bath as only an occasional and uncertain adjuvant:—whether we have

a surer and better substitute in the proposal now made, of introducing the same agent into the lungs, yet remains to be known.

As, in this disease, the burning sensation at the pit of the stomach is so great, and the thirst so urgent that it is with difficulty the patient can be debarred from partaking freely of whatever fluid is within his reach; and as so much difference of opinion exists on the subject, some explanation will naturally be expected on this point. It must be familiar to all, that, in this country, copious draughts of cold water were employed as a remedial agent in this disease. The success attending the practice was, doubtless, to be ascribed to the emetic property of the remedy; a success common to many other emetics. But although numerous and highly interesting examples are recorded of recovery by these means, in apparently hopeless cases, subsequent experience and a more extended trial proved that, in others, the reaction which took place, after recovery from the collapsed stage of the disease, was greater and more dangerous than when this practice was not adopted. In India, if we credit the united testimony of every practitioner there, when cold drinks were allowed, and when the patient, in spite of the instant rejection of the fluid, persisted in its use, the same result which has been frequently witnessed in England was seldom observed there—the recovery of the patient. On the contrary, as Dr. Young has remarked, if this craving for cool drinks was indulged in, the frightful

state of collapse soon succeeded, and all the bad and dangerous symptoms were immediately increased. It is not surprising, therefore, after witnessing such results, that a total abstinence from all liquids was strictly enjoined by the majority of the profession in that country; and only administered by others in the smallest possible quantity, as a table spoonful at a time, until the worst symptoms had disappeared.

The danger arising from the use of cool drinks in India may, probably, be ascribed to the more rapid absorption of fluids in that country when introduced into the stomach, and their conveyance onwards to the venous centre, carrying with them fresh portions of the morbid matter. As, however, in colder climates the tendency of the same matter is more towards the intestinal canal than the lungs, such a result would be less likely to be witnessed. On the contrary, the vomiting which is produced at each draught (a result always attendant upon this practice in England) will act as a powerful agent in favouring the propulsion of the poison out of the system, and thus prevent or remove the collapse itself.

With such different results before us, and such a diversity of opinion, it might be somewhat difficult to give a safe guide or general rule on the subject. But being fully convinced that the great and urgent thirst, in this disease, can be relieved by other more certain and less doubtful means; and believing that it is not requisite, or desirable, to

resort to such a remedy on account of its emetic effect, the necessity for any rule is, in this case, entirely done away with. In administering carbonic acid, in this disease, the burning sensation in the region of the stomach, and the great and urgent thirst, are not only more effectually relieved than by any other means, but the latter sensation is speedily and, generally, entirely removed by this simple and single remedy. In giving or withholding any liquid, I have been guided entirely by the patient's own wishes and feelings, not having seen any harm result from the use of simple diluents, as toast and water, so soon as one or two doses of the medicine have been exhibited. On the contrary, in the evacuant period of the disease, but more particularly after the cessation of the intestinal discharges, it is advisable, and necessary, to encourage the patient to partake freely of liquids, in order to supply the place of the discharged serum and restore the lost fluidity of the blood.

Lastly, as regards that universal medicine, mercury. Knowing the influence which this remedy exerts on all the secretions, and the specific action which it produces on the capillary system,—were I obliged or induced, by any cause, to resort to the treatment by expulsion, mercury, in some one of its forms, would be my principal resource. But being satisfied that there is an agent capable of neutralizing the morbid matter of this disease; and that the means we possess for the expulsion of

the poison are uncertain in their operation, while it is clear that calomel itself undergoes no change when introduced into the stomach in the stage of collapse,—the use of this agent is necessarily limited to one purpose—the exciting the flow of bile, and aiding in restoring the suspended secretions. But as the arrest of the flow of bile, and the suspension of the other secretions, are but effects of one common cause—the action of a poison in the system—it must be more reasonable to wait until that cause is removed, before attempting to remedy the effects which have been produced by it. The writer of the Madras report, in reference to this practice, remarks, “Calomel has unquestionably a powerful effect in exciting the biliary system, and, in this view, its exhibition is highly necessary; but the suppression of the excretion of bile being only a link in the common chain of symptoms; and its partial or occasional removal, or even its total absence, having been proved to be of little consequence in the general course of the disease, to attempt to excite it by particular means may be considered as premature and injudicious.” In this view of the subject every reflecting person must concur; and believing that I have an agent capable of removing the cause of the suppression of the various secretions, my plan has been to wait until reaction has commenced before resorting to calomel, which can only be regarded as an adjuvant, a powerful one it is true, but still only of secondary and not primary

importance in the list of remedial agents. As, also, it is an object to clear the system of those excrementitious matters which, by the suspension of the secretions, have been retained in the system, some purgative medicine, as castor oil, should likewise be taken ten or twelve hours after the administration of the calomel.

FINIS

APPENDIX.

IN order to confirm what has been advanced respecting the efficacy of carbonic acid, a few cases illustrative of the effects of this remedy, in the different stages of the disease, will now be added.

CASE I.—A factoryman was attacked, during the prevalence of the Epidemic, with the diarrhoea, which was, at that period, the common precursor of the more severe attacks. At the time of his application for relief, twelve hours after the commencement of the purging, the evacuations were watery and rather abundant, but still frequent. There was neither nausea nor vomiting, but the patient complained of listlessness, and, when questioned, of a slight feeling of oppression and sinking at the pit of the stomach; the pulse, also, being slow and weak, and the physiognomy slightly altered.

He was ordered a simple effervescent draught, with the addition of a little syrup; to be repeated every hour, until the purging had ceased. The first dose removed, in some measure, the intensity of the above symptoms, and the diarrhoea ceased soon after the administration of the third dose. Another draught completed the cure.

CASE II.—The cook in a respectable family, many of the members of which had previously suffered from the disease, and at a time when the Epidemic was still prevailing to a considerable extent in the neighbourhood, was suddenly attacked with diarrhoea, together with nausea, and a feeling of lassitude and sinking at the pit of the stomach. Being in the house at the time, I prescribed an effervescent draught, and ordered

it to be repeated every two hours until the purging ceased. The first dose completely relieved every unpleasant symptom; and as the patient considered it unnecessary, under these circumstances, to resort a second time to the medicine, no more was taken. Notwithstanding this, the diarrhœa did not return.

In one case, in which the diarrhœa had existed for eight or ten days previous to the exhibition of the carbonic acid, although the colour and consistency of the stools were altered by the administration of the remedy,—being less watery, more feculent, and indicating an increased flow of bile,—the relaxation still continued in a slight degree. The carbonic acid, therefore, was discontinued after four doses had been taken, and an enema of charcoal administered, which sufficed to check altogether the purging.

In another case, in which a similar occurrence was observed after the administration of six doses of the gas, instead of the pure carbon the carbonate of lime was given, with the like result.

Although the above two cases have been quoted, in accordance with the remarks before made respecting the treatment of the disease, at this period, I ought to observe, that there has seldom been occasion to resort either to carbon or the carbonates, when the carbonic acid has been given at the commencement of the attack, and when there existed no particular predisposition of the individual to relaxation of the bowels. It was principally in Spain, with patients who had been labouring under premonitory diarrhœa for eight or ten days, living upon rice-water and taking strong sudorifies; and in whom, in addition to this debilitating treatment, there existed also a natural tendency to relaxed bowels, that I had occasion to employ the remedy in the latter mentioned form.

CASE III.—A lady who resided in a neighbourhood where the Epidemic was prevailing to a considerable extent, and, while apparently in the enjoyment of her accustomed health, suddenly experienced those symptoms which have been described as preceding the more severe form of Cholera. These were a sinking and oppression at the pit of the stomach, nausea and giddiness, succeeded by agitation, nervousness, and a sense of burning or heat in the epigastric region. When visited by me a short time after, in addition to the above symptoms, her pulse was found to be slow, weak and fluttering.

Having ordered an effervescing draught to be taken immediately, a few minutes only had elapsed before the patient expressed herself greatly relieved. A second dose, repeated in half an hour entirely removed the nausea and burning sensation at the pit of the stomach. But as the

nervousness did not appear to be quite gone, a third dose was ordered to be given in an hour, after which every unpleasant symptom was dissipated, and the patient restored to health.

CASE IV.—In June 1832, I had been undergoing some fatigue in visiting several Cholera patients. During my attendance upon one of them, I was suddenly attacked with nausea, succeeded by faintness and giddiness, which obliged me to take hold of the table for support. Having been previously walking rather fast, I attributed these symptoms to over exertion; and as they were speedily dissipated, and my attention engaged at the time by other circumstances, nothing was taken for their relief. On my return home, which was not until late, I felt rather nervous, depressed, and languid, but still thinking it was to be referred to the additional fatigue I had undergone, while in a state of delicate health, I partook of a slight supper, with the addition of a glass of brandy and water, and retired to bed.

About three hours after, I awoke with a sense of oppression at the chest, giddiness, and faintness, while the surface of my body was covered with a cold clammy perspiration. The pulse was small, slow, and tremulous, and the desire to vomit so great that it was with difficulty I resisted the effort, which I was induced to do from a feeling of inability, at that moment, to raise myself in the bed. While revolving in my mind what steps to pursue, I felt my bowels strongly and powerfully excited, and, upon getting up, passed a profuse, watery, but still feculent motion, accompanied and succeeded by those sensations so well and accurately described by the writer of the Madras Report. The entire intestinal tube seemed to be at once emptied of the whole of its contents; and an indescribable, but most subduing feeling of exhaustion, sinking, and emptiness was produced.

Having some soda and acid in the room, I took an effervescing draught, which speedily removed the unpleasant symptoms with which I was at first attacked, viz. the nausea, giddiness, faintness, and oppression. In about half an hour, I was again disturbed by a desire to go to the night stool, when a quantity of fluid, characteristic of the disease, being colourless and of the appearance of rice water, was discharged.

The effervescing draught being repeated, my bowels continued quiet for an hour, when a similar evacuation was passed, but not so abundant.

A third dose of the medicine was then taken, and the bowels remained undisturbed for two hours, at the end of which period they

were again acted on, but to a very slight degree. Another dose entirely removed the relaxation.

During the remainder of the day I appeared weak, languid and nervous, and as if my system had received a severe shock. In the evening, these symptoms not being dissipated, recourse was had to the same draught as before, with the effect of relieving them entirely. A mild alterative and a slight tonic restored me, in a few days, to my accustomed health.

CASE V.—A practicante, or assistant, in one of the Cholera Hospitals in Spain, had been suffering, for several days, with preliminary diarrhœa, for which he had been treated on the antiphlogistic principle, viz. leeches to the abdomen, emollient drinks, &c. to which had been added one general bleeding from the arm.

The purging had ceased, and the patient was apparently recovering from his indisposition, when he was suddenly attacked with severe purging of a thin, transparent, or congee-looking fluid, attended with considerable spasm. The pulse was small and fluttering; the countenance expressive of anxiety; and the physiognomy slightly altered. He was soon after seized with vomiting, the fluid thrown up being similar in appearance to that evacuated from the bowels.

Being in the Hospital at the time (making a visit with the attendant physician) I advised the exhibition of some effervescing draughts, which being agreed to, I had the satisfaction of seeing the patient partly relieved, and the vomiting checked after the administration of the first draught; and of hearing, in the morning, that the purging also was arrested, and every other unpleasant symptom removed, after three more doses had been taken.

CASE VI.—A lady, upwards of sixty, during the second invasion of the disease in London in the summer of 1832, in common with so many at that time, had been suffering with diarrhœa for several days. On the day preceding the attack which I am now about to relate, the diarrhœa ceased, and the patient had retired to bed congratulating herself on her apparent recovery. About two hours afterwards, she was attacked with the premonitory symptoms of the severe form of Cholera; but as the nature of the disease was not clearly understood by the friends and attendants, some brandy and water was given, under the supposition that indigestion was the cause. The symptoms, however, having increased in violence, and vomiting coming on, I was summoned about an hour after the commencement of the attack.

A rather severe vomit had been experienced previous to my arrival, succeeded by considerable jactitation the patient, at the moment I saw her, throwing herself incessantly from side to side of the bed, while her countenance was expressive of the greatest anxiety. Having been again seized with vomiting, a quantity of fluid of the characteristic colour and appearance was thrown up, after which an effervescing draught was given, composed of carbonate of potash and tartaric acid. This had the effect of allaying, in a great measure, the restlessness and agitation, and of procuring, at the same time, any return of the vomiting. But as the patient still complained of nausea, together with oppression at the chest, and a sensation of heat and burning at the pit of the stomach, a second draught was given, in ten minutes, which entirely removed these symptoms.

About the same time the bowels were suddenly acted on, the evacuation being watery and of the diarrhetic appearance, but slightly tinged with bilious matter. The patient then remained quiet and tranquil for an hour, when another evacuation, but without any fecal matter, and of the colour of rose-water, was passed. The draught was repeated, and the patient soon after fell into a quiet and comfortable sleep, and so continued for nearly two hours.

At the end of this time, being in the last room, I was alarmed by the sudden colour of the patient, who was aroused from sleep by severe cramps in both legs; the bowels also being, shortly after, again acted on. In addition to this, I was grieved to observe that the agitation and restlessness had also returned, that the veins of the face were distended, and that around the mouth and eyes there was a blue circle, the finger-nails also being of the same dark tint. This blueness soon increased over the face and hands, while the skin of the fingers became shrunk and wrinkled.

These symptoms, indicating the commencement of the stage of collapse, nullified my regret having allowed so much valuable time to pass by without using any means to prevent the further progress of the disease. A draught the same as before, but with the addition of five grains of the carbonate of ammonia, was immediately given, and repeated in ten minutes. The restlessness and agitation disappeared after the administration of the first dose; and the cramps, although returning slightly at intervals, gradually became less and less, and had entirely ceased before the next dose of the medicine was given, and which the patient took half an hour after the administration of the previous draught.

The other symptoms then appeared to remain stationary; but in about half an hour from the commencement of the cramps, the pulse began to rise in strength and frequency, the lividity and blueness to disappear, and the skin to lose its wrinkled and par-boiled appearance. Up to this period the medicine had been continued every half-hour; but, as the above symptoms were quickly removed, so as to leave the patient, at the end of between two and three hours, quite free from all ailment, and without any appearance of having passed through the collapsed stage of Cholera, with the exception of slight turgescence of some of the superficial veins of the face and extremities, it was not thought necessary to continue the medicine. The next day the patient was convalescent; and, in a short time, by the assistance of a slight course of tonic medicine, regained her accustomed health, and experienced no relapse.

Obs.—This case has been narrated, not as the best which could have been selected, in proof of the superior efficacy of carbonic acid in the Epidemic Cholera, but as illustrative of the theory which has been broached, respecting the physiology of this disease; for it shows that the administration of the remedy, in sufficient quantity to relieve the symptoms common to the first stages of the disease, and referrible to derangement in the alimentary canal, did not, nevertheless, prevent the supervention of the stage of collapse. As, however, this subject has been already touched upon, it is unnecessary now to say more than that, independent of all theory, an attentive observation and more extended experience, since that time, enables me to affirm, that, had the administration of the remedy been continued at regular intervals in the previous stage, those symptoms characteristic of the state of collapse, which subsequently appeared, would have been altogether and entirely prevented.

CASE VII.—A female, of middle age, but of robust constitution, was brought into the Hospital of St. Pablo, Barcelona, in the commencement of the stage of collapse. The vomiting was incessant, and the evacuations frequent and abundant; the patient suffering, at the same time, from the most severe and distressing cramps. No treatment had been adopted previous to the arrival of the patient in the Hospital, when frictions were ordered, to allay the violence of the spasms, but without any avail, although three or four assistants were employed in the operation, and stimulating embrocations also resorted to. She then commenced to take the carbonic acid, the first dose of

which was sufficient to arrest the vomiting, and relieve, in great measure, the sensation of heat and burning in the epigastric region. A second dose, administered at an interval of a quarter of an hour, not only entirely dissipated these symptoms, but removed, at the same time, the spasms also. After four more doses had been taken, the purging was arrested, and the depression of the nervous system, as well as all other symptoms common to this stage, entirely removed. In a few days, the patient was convalescent.

Although it is somewhat rare to find children attacked with this disease, it is a pleasure to me to be enabled to state, that the plan of treatment here recommended can be adapted with the greatest facility, even with the youngest infants. Of five children lying in the Cholera Hospital, Barcelona, at the same time, four were rescued from an untimely grave by the administration of carbon, and carbonic acid, notwithstanding that the state of collapse had already set in. Two of these children, who had been removed from the Foundling Hospital, where they suffered severely from the spasms, were too young to be induced to take the necessary draught. The pure carbon, therefore, was employed in the form of stercor, which sufficed to check the progress of the disease, and remove the state of collapse, as well as all other morbid symptoms. In cases of a milder nature, when collapse exists, and when we are prevented administering the gas by the mouth, I should always recommend, if it be practicable, the injection of carbonic acid into the rectum, instead of, or in combination with, the charcoal; as the effect, in this case, would be more prompt and certain. The same proportions of soda and acid, and administered at the same intervals, as when taken into the stomach, may be used in these instances also.

CASE VIII.—Passing through the wards of one of the Cholera Hospitals in Spain, the assistant was summoned to a family, who had been suddenly seized with severe cramps, after the symptoms common to the first and second stages, through which the patient had passed, had been removed by ordinary means. I prevailed upon the gentleman to administer a few doses of carbonic acid, merely with the view of ascertaining the remedial power of the antilete in this particular instance. It was a source of satisfaction to me to find, at my next visit, that the spasms had quickly yielded to the internal administration of the medicine, without its being necessary to resort to any adjuvant.

CASE IX.—A convalescent in the Royal Naval Hospital, Woolwich,

was suddenly attacked, in the summer of 1832, with the Cholera in its severest form, having been found by Dr. Parkin, who arrived ten minutes after, without pulse, and in a state of collapse.

This gentleman, to whom I am infinitely indebted for the many valuable facilities afforded me in the prosecution of my inquiry, observing in the ward some bottled porter, the allowance of one of the other patients, and thinking that the individual attacked had but a short time to live, immediately ordered the cork to be drawn, and made him take a large draught. The effect was almost instantaneous, for the patient, throwing himself back, and drawing a deep inspiration, began immediately to revive; and re-action took place, almost as quickly as the collapse had come on.

Although no vomiting or purging was observed previous to the attack, the patient passed, subsequently to re-action taking place, several stools characteristic of the disease. For this the pure carbon was had recourse to, and for the removal of the remaining symptoms small doses of calomel were employed, with the effect of restoring the patient, in a few days, to his former state.

Two cases treated by Mr. Radcliffe, of Brentford, and inserted in the *Lancet*, Sept. 29th, 1832, also shew in a striking manner the power of the same remedy, at this stage of the disease.

CASE X.—The first case was that of a strong muscular man attacked with purging, during the prevalence of the Cholera in that town. Mr. Radcliffe being sent for, the patient was immediately bled, but it was with difficulty that eight ounces of dark grumous blood could be obtained. He was then seized with cramps, and had a rice-water stool, which was shortly followed by vomiting, causing him to reject immediately some pills which had been given. "As it was evident" that no re-action would be produced by the bleeding, but, on the contrary, that collapse was fast approaching, strong brandy and water "was given frequently, but always rejected by vomiting; frictions of the" extremities were also perseveringly used, but without any good effect; "and at 7 P. M. the pulse at the wrist became imperceptible, as also did" the action of the heart." With the view of allaying the sickness, an effervescing draught was now given, containing five grains of the carbonate of ammonia in excess. "This the patient swallowed with" avidity, and it completely allayed the vomiting, so that he retained the "two-before mentioned pills which were given along with the draught." Frictions were then used, and sinapisms applied, while the effervescing draught was continued every half hour. "At 10 P. M. (three hours after

" the commencement of collapse, the pulse was still imperceptible, so
 " that the draughts were continued every hour for three hours, as well
 " as the frictions, by which thae re-action commenced, and at half-past
 " 6 A. M. the pulse had risen to ninety, and the heat of surface equally
 " restored." Simple effervescing draughts were then given, without the
 carbonate of ammonia, every two hours; and grain doses of calomel
 every hour, until the secretions resumed their usual character. " The
 " consecutive fever, which was rather high, was treated upon general
 " principles, and, in six days, the patient's health was re-established "

CASE XI.—The next case was that of a child, *æt.* four years, who
 was seized at midnight with passing and vomiting of a fluid resembling
 rose water. This continued for three hours, at the end of which period
 he was seen by Mr. Radcliffe, who found him in a state of collapse,
 and suffering from cramps. " At 7 A. M., the sickness continuing and
 " the pulse being still imperceptible at the wrist, an effervescing draught,
 " containing two grains of carbonate of ammonia in excess, was given,
 " which completely allayed the vomiting, and enabled him to retain a
 " calomel powder. Frictions were continued, and the effervescing
 " draughts repeated every half hour, until thirteen doses had been taken,
 " when re-action took place to more than the desired extent, as the pulse
 " soon rose to 120, and the head became affected. At this period, con-
 "secutive fever alone remained, which was treated with mild aperients
 " and effervescing salines." On the 2^d (four days after the attack) the
 " patient was convalescent.

(11c).—The above two cases have been inserted on account of their
 being the only published histories yet extant, which I have met with,
 where the carbonic acid was the principal or the sole remedial agent
 employed. The circumstance that each of the two patients took a dose
 of calomel, in combination with the above remedy, cannot possibly
 deduct, in the least, from the value which ought to be attached to the
 carbonic acid. Had it been necessary, I could have quoted dozens of
 cases of a similar nature, in which, after the failure of blood-letting
 and other means, the carbonic acid was given alone, and with a similar
 result. One public case, however, from an unbiased and unprejudiced
 person, appeared of more value, in my eyes, than a dozen others given
 by one who might be considered to have a partial feeling on the
 subject.

R E P O R T S.

In addition to what has been already advanced respecting the efficacy of the different forms of carbon, it affords me the highest satisfaction to be enabled to give the following confirmatory and more valuable evidence from the pen of several of my brother practitioners.

In the narrative of the Cholera, published in the *Lancet*, it is stated that "an exceedingly simple remedy was used; and, it is said, "with unprecedented success, on board the ships belonging to the United States. A common bottle cork was burnt, and the powdered coal "given in a little milk or water. The third dose, at most, was sufficient "to allay the urgent symptoms; and we are assured that it has more "than once saved patients, almost in the agony of death."

At the time when I was following up my own observations in England with the same substance, the simple or pure carbon was also given by M. Bielt, at the Hospital of St. Louis, during the prevalence of the epidemic in Paris; but was soon relinquished again, on account of its apparent inefficacy. This failure, in all probability, arose from the circumstance that the carbon was not prepared at the moment of its being used; in which case, as I have myself experienced, the beneficial effects of the remedy are not witnessed.

At a period later than this, and subsequent to my promulgation of the effects of carbon in this country, we received accounts of the success which an individual had met with in Canada, by the employment of the same remedy. It was during the height of the epidemic that this singular being, clad in a most grotesque fashion, and driving as singular an equipage, appeared in one of the Canadian towns. As medical men are somewhat scarce in that country, and as he pretended to be in possession of a certain cure for this disease, he was not long in want of patients. It appears that he prepared the remedy at the moment, and in the presence of his patient; using certain forms and ceremonies, which, although ridiculous in themselves, were not altogether without benefit. Partly in consequence of the mystery of his manner, and the belief that, by these ceremonies, he charmed away the disease, (a supposition readily entertained by the vulgar and ignorant) but principally in consequence of the invariable success of his plan of treatment, he inspired, as it would seem, almost universal confidence among those who had an opportunity of witnessing his

practice. This confidence was not misplaced; for, however, we may laugh at his ceremonies and incantations, it was allowed by all who have given us any particulars respecting him, that he lost few or no patients by this method of treatment. The true charm consisted in the preparation of the remedy at the moment of its administration; for, as was before remarked, carbon when recent, possesses properties which loses by age.

Again, by a letter from M. Moreau de Juvenet, inserted in the *Lancet*,* we learn that Dr. Gavarret, of Arras, had administered a table spoonful of wood charcoal, in an agitated carmin, during the prevalence of the Cholera in the Pas de Calais. In twelve cases of which he gives the details, the cure was complete and immediate. In several others he omitted the opium, with the same success.

In consulting the practice of different individuals, and perusing the documents transmitted to the central Board of Health, it will be discovered that, after the first appearance of the disease in England, carbonic acid came to be more and more generally adopted, to allay the irritation of the stomach and check the vomiting, not only in the first stages of the disease, but in that of collapse also.

Thus, Mr. Woodhouse, of St. Thomas's, near Exeter, states, in his report, that when the vomiting continues urgent, he has given the saline draught, in a state of effervescence, with great advantage.

Soda water, solution of soda, and effervescing draughts, were also used in the treatment of the disease by the surgeons at Calcutta, near Selly.

The medical practitioners at East Retford remark, that they found vomiting best relieved by soda powders, in a state of effervescence, and thirst, by cold water or soda water.

Dr. Stevens, so well known as the proposer and advocate of what has been termed the saline treatment, has the following remarks appended by way of note, to a paper respecting the above method:—
 “When the stomach is irritable, which it generally is in Cholera,
 “(who could have doubted it), the saline effervescing draughts are of
 “great value; and I feel confident that the mortality from this disease
 “would be greatly lessened, even if we were to trust almost entirely to
 “this simple remedy.”†

Although there was only one other English practitioner besides myself, in Spain, during the prevalence of the Epidemic in that country,

* November 11th. 1831.

† Medical Gazette, August 18th. 1831.

it was a source of no small satisfaction to me to find that we were both of the same opinion as regarded the treatment of a disease, respecting which so many plans have been proposed. Dr. Wilson, a retired army surgeon, resident at Xeres during the eruption of the disease in that town, was induced to give a trial to the pure carbon, in the treatment of the numerous cases which came under his care, at that period. Although this gentleman did not confine himself exclusively to this single remedy, but occasionally employed olive oil, (an agent much resorted to in the south of Spain as an emetic) together with calomel and bismuth, in the collapsed stage; yet, as, in the majority of cases, this remedy was the principal, if not the only agent, employed; and as, in addition to his own exertions in numerous instances, Dr. Wilson had two or three English assistants, (gentlemen not of the profession) who nobly volunteered their services on the occasion, and who confined themselves almost entirely to the administration of carbon, it may be said that many thousands were cured by this remedy alone.

Dr. Wilson having kindly favoured me with the notes he made on that occasion, it is a source of much regret, that the non-arrival of the box containing my books and papers, which I left in Madrid to be forwarded by a different route to that I took myself, prevents my giving any extract from this interesting memoir. I have the author's permission, however, to state, that he had arrived at the same conclusion as myself, respecting the remedial virtues of the pure carbon, viz. that it is an unfailing remedy for the disease, when given before the state of confirmed collapse. Experience, however, soon taught this gentleman the necessity there was to prepare the remedy in a proper manner, and to keep it in well-corked bottles, free from the contact of the external air; as also not to administer any but what had been recently prepared. For this purpose, several persons were kept constantly employed, during the whole period of the prevalence of the disease, in preparing the charcoal,—a practice well worthy of imitation, in instances when it is not practicable or convenient to administer the carbonic acid.

Greatly as I am indebted to Dr. Wilson for his kind permission to make an extract from his MS., and to use his name in any way I may think proper relative to the opinion he has formed of the efficacy of the pure carbon, I should be still further pleased if this gentleman would comply with my wishes, and communicate to the profession, in some other way, the result of his treatment of the disease in Xeres; as I am convinced such a communication would not only be

valuable in itself, but be received, by the faculty, with that consideration which is due to an individual who had previously distinguished himself in the treatment of a disease, little less fatal than the Epidemic Cholera,—I mean the Yellow Fever.

In answer to some observations which were made in the *Vapor* newspaper of Barcelona, relative to the effect of carbonic acid in one particular case, the attendant physicians, Dr. Ardevol, Dr. Frau, (Professor of Anatomy and Surgery) and Dr. Sanja, (Secretary to the Academy of Medicine) who were the first individuals that adopted the plan of treatment here proposed, in that town, concluded their letter in these terms:—"This chemical agent is a specific remedy for
" the cure of the Asiatic Cholera, in its first and second periods; ad-
" ministered by skilful hands and with medical tact. We have the
" satisfaction of having saved, with this remedy, dozens of individuals
" who were brought to the gates of death."

The former of these gentlemen, in a subsequent communication, published in the *Cabales*,* *periodical of Barcelona*, offers the following strong testimony to the efficacy of this remedy:—"The carbonic acid
" gas," observes Dr. Ardevol "is a chemical agent which positively
" neutralizes the malarious poison of Cholera. Its effects are observable
" in the first, second, third, and fourth periods of the disease. In that
" of the preliminary diarrhoea, it modifies the morbid impression in a
" perceptible manner; changes the nature of the discharges, turning one
" to bilious secretions; and produces a speedy alteration in the choleric
" physiognomy of the patient, who soon resumes his normal appear-
" ance."

"The efficacy of this Medicine," continues the writer, "is most
" visible at the commencement of collapse, when the bluntness appears,
" and when there is an alteration and sinking of the voice. The medical
" metamorphosis appears most remarkable in such othermost desperate
" cases, since the physician observes with pleasure the speedy effects of
" the neutralization of the poison, by the disappearance of the anxiety,
" by the recovery of the voice; animation of the physiognomy; return
" of the pulse, and removal of the state of depression; and, finally,
" remarks that the patient passes to the period of reaction with a free
" development of the pulse, confirming thereby the hopes of the suf-
" fering, for the re-establishment of his health. In the apical state, with
" bluntness, loss of pulse, suppression of urine, and visible depression,

* No. 41. 22^{da} November 1834.

“ if the patient preserves his intellectual faculties, its good effects are
 “ also observable to all who have sight and wish to see. And I will say,
 “ finally, that, in the actual state of our knowledge, the *materia medica*,
 “ in this case, has no other agent which can replace it.”

This document, which I appreciate as the dying testimony of an individual for whose memory I shall always entertain the greatest respect, and to whom I was indebted for much valuable co-operation during my stay in Barcelona, will not fail to make an impression on the mind of every unprejudiced person, coming as it does from a gentleman of reputed talents in his profession, and who to his general experience, acquired in different parts of the world, added the advantage of having witnessed the fatal eruption of the Epidemic Cholera in the capital of France. But lest my motives should be suspected in paying a just tribute of praise to the memory of a departed friend, I will content myself with re-echoing the words of the Academy of Medicine in Barcelona, on occasion of awarding a gold medal to this lamented physician, a short period before his death, for a treatise on the yellow fever. “ His observations,” concludes the report, “ have
 “ that force and exactitude which prove and convince, at the same time
 “ that the precision with which he describes the three periods distinguishing the disease, causes a certain analogy to appear with the
 “ method which the great Hippocrates preserves in his invaluable works,
 “ *de morbis popularibus*, and which makes this part of the treatise highly
 “ recommendable. Lastly, the memoir is founded on data collected at
 “ the bed-side of the sick ; and while those minutiae worthy of a
 “ skilful practitioner and investigator are not omitted, it abounds in
 “ original ideas as to the curative treatment which experience happily
 “ suggested to the author.”*

But it is to the medical practitioners of Mataro, the Brighton of Catalonia, I am most indebted, for the assistance which they have afforded to that cause wherein I now find myself engaged. This will appear evident by the perusal of the following extract from an article inserted in one of the Medical Journals of Madrid, by Dr. Pascual.†

It commences thus,

“ The continued announcement of specific remedies for counter-

* The name of Dr. Ardevol must be familiar to many of the English surgeons stationed in Gibraltar during the prevalence of the yellow fever in that fortress. He was also much appreciated in France, where he distinguished himself, while graduating at Montpellier by several interesting and original essays.

† Boletin de Medicina y. Cirujia, No. 35, Jan. 29, 1835.

“ acting the effects of this destructive disease ; and their inefficacy, in-
“ utility, and detriment in the hands of the physician who has trusted
“ to the unbounded praises with which they have been enhanced, will,
“ perhaps, cause this new remedial agent to be viewed as another of the
“ numberless advertizements which have adorned the corners of the
“ streets ; filled the columns of the newspapers ; and, lastly, bar-
“ barously trafficked with the credulity and ignorance of the vulgar,
“ always easily beguiled and never tutored by experience. Impressed
“ with a conviction of the necessity and obligation by which the clinical
“ observer and historian is bound, to use the language of sincerity
“ and freedom ; and impelled solely by a desire to be useful to humanity
“ and science, I am going to present the result of the administration
“ of carbonic acid gas in the Cholera Morbus of the town of Mataro,
“ from the eighth day of October to the 12th of December, 1834.

“ In the first days of the appearance of the disease, all the patients
“ were treated with the method entirely antiphlogistic ; but three or
“ four persons of note who were subjected to this treatment having
“ perished, it lost credit, and caused bleeding to be regarded as the
“ passport to the other world.”

It was at this period that I placed myself in communication with the physicians of Mataro, who having heard that the administration of carbonic acid had been attended with considerable success in Barcelona, were easily induced to try its effects in that town also. “ This,” continues the narrator, “ happened in the period of the ascent
“ of the disease. But what was our pleasing surprize, when we really
“ saw all the patients who summoned us before the state of collapse
“ became developed, change, *as by enchantment*, their morbid condition,
“ under the immediate influence of the gas ! You may be assured,
“ Gentlemen, that I do not magnify the facts ; but it is necessary to
“ have seen the effect in order to believe it. The several practitioners
“ mutually recounted to each other the pleasing change which, with
“ satisfaction, they observed in the sick ; so that we were able to prog-
“ nosticate, with all the probability of which the science of medicine
“ is susceptible, that a patient who took carbonic acid, before the
“ period of asphyxia, generally recovered speedily and securely.

“ The most immediate effects which I have constantly observed
“ from the introduction of the gas into the stomach, have been the
“ following. If the patient is labouring under suspicious diarrhœa ;
“ loud rumbling of the bowels ; small pulse ; slight alteration in the

“ physiognomy and voice somewhat changed,—the third dose usually
 “ suspends altogether the diarrhœa, and the other symptoms disappear
 “ successively—the course of the disease, consequently, being arrested.
 “ If the patient has that continued mortal oppression at the præcordia,
 “ with burning heat in the epigastric region; vomitings, and rice-water
 “ evacuations; remarkable alteration in the physiognomy, without having
 “ altogether lost the temperature of the extremities; it may be said that
 “ *the carbonic acid*, in these causes, *acts miraculously*. At the third or
 “ fourth dose the patient experiences a calm and indescribable ease,
 “ which makes him exclaim, with all the vehemence imaginable, *Give me*
 “ *more of this draught, for it restores me to life*. Truly it was surprising
 “ to see the rapid amendment of a patient, a little while before in so
 “ dangerous a state. The purging either ceased, or was converted from
 “ albuminous into excrementitious; the voice, the pulse, the physi-
 “ ognomy, recovered themselves remarkably, and the physician inwardly
 “ rejoiced, at the promptitude, security, and gentleness, with which he
 “ had just snatched from the jaws of death an idolized father, a beloved
 “ son, or a mother surrounded with presumptive orphans.

“ Do not suppose, gentlemen, that I overcharge in any respect this
 “ medical recital. Fourteen practitioners would, I am certain, put their
 “ signature to any testimony which might be required from their honour.
 “ Persons of the first rank in this city proclaim openly the advantages
 “ which they experienced from this remedy; and I, also, should consider
 “ myself unworthy of the noble profession which I exercise, if I had been
 “ capable of distorting, in the least, a relation, whose object is no less
 “ than the good of humanity, and the advancement of medical science.
 “ All the objections which can be made against the especial efficacy of
 “ carbonic acid in the Cholera Morbus of this city, are thrown down at
 “ the feet of the appreciable multitude of practical facts that I, the other
 “ practitioners, and, in particular, Dr. Rabasa, physician of the hospital,
 “ have collected, unless it should be proved that we have not seen that
 “ which we have seen.*”

It is unnecessary for me, even were it becoming, to express any

* These documents, together with a copy of the two Spanish editions of this work, will
 be lodged in the Library of the Royal College of Surgeons, London; where they will be
 open to the inspection of any individual who may wish to peruse them. I should also state,
 that the French edition, containing the first part only of the Spanish work, and which was
 translated by Dr. Dunal, of Montpellier, may be had of M. Alexandre, the French Medical
 Bookseller, Great Russell Street, Bloomsbury.

opinion on the value of this report; neither will I attempt to lower the dignity of that profession to which we individually belong; or cause the notices of these gentlemen for coming forward on this occasion to be suspected, even for a moment, by presuming to offer them the thanks, on the score of an humble individual and fellow-labourer in that cause in which we are now mutually engaged.

It may be permitted for me to remark, nevertheless, that the conduct of the physicians of Mataro affords us an example of liberality and unanimity of opinion so rare as it is precious, in persons of the same profession; while, I trust, it will not be considered egotism on my part to add, that the history of the remedy in Mataro stands almost alone in the annals of medicine; since it is probable that few other examples could be adduced in which the whole of the medical practitioners of a town, without a single exception, have, after a few days' trial, unanimously adopted a particular remedy, to the exclusion of every other, for the treatment of a devastating and previously intractable sickness.

I had been in hopes that a small memoir, drawn up by Dr. Raimon, physician to the general hospital in that town, would have been published ere this; such having been the intention of Dr. R., as he informed me when we parted from each other at Mataro; I was, therefore, not so particular in collecting data, as I should otherwise have been, with regard to the number attacked, and the mortality. I understood, however, that the individuals attacked amounted to upwards of a 1,000, and the deaths to about 60; of this number between 20 and 25 died previous to the employment of calomel used, in that town. That the small mortality, in this instance, did not arise from the mildness of the disease, may be presumed from the fact, that in an official answer received by me from the Board of Health in Mataro, to some inquiries I had made as to the extent of the disease, it was stated, the number of cases had then amounted to 20, and the deaths to 15, being just half. In addition it may be stated, that during the first visit which I paid to Mataro, of six patients lying in the general hospital for the town being principally composed of respectable inhabitants, it was thought unnecessary to establish a cholera hospital; 5 either had been, or then were, in a state of collapse. From the number of private cases also which I saw, in the same state, I consider that the type of the disease was as severe as in Barcelona, if not in Valencia, where the mortality exceeded in the latter town, 6,000, and, in the former 4,000,—a rather large average for the population,

reduced to nearly one-half by absentees and amounting, as it was calculated, to 50, or 60,000 in each place.*

In addition to the document written by Dr. Pascual, in the name of himself, and with the approbation of the other practitioners, another report was also sent by the whole of these gentlemen to the Supreme Board of Health, in favour of the same practice. The Academy of Barcelona, likewise, made favourable mention of the specific properties of carbonic acid, in the report sent by them to the Supreme Board of Medicine, in Madrid, on the treatment of the Epidemic in the above towns. Two other very strong reports were also drawn up by the several practitioners of the towns of Salamanca and Ballecas in favour of this remedy.

It having been the intention of the Spanish government to publish a narrative of the different plans of treatment adopted in that country, during the prevalence of the Epidemic, and their result, wherein the one which forms the subject of the present work was to have been included, I waited for some time in the hope that this intention would have been carried into effect.

Having been disappointed in this respect, I forwarded an application to the proper authorities for permission to make extracts from these several reports; and having obtained this, I spent three or four weeks,—fruitlessly, I am sorry to add,—in endeavouring to discover them among several hundreds in the archives of the Minister of the Interior, of the Supreme Board of Medicine, and the different Boards of Health. It then only remained to search the archives of the Academy of Medicine of Madrid; but, as it was inconvenient to me to remain longer in that capital, I was obliged, much to my regret, to leave this task to the secretary, who kindly promised to execute it, and forward me a copy of such of the reports as were to be found, either in the library, or in the possession of any of the members, who had

* Speaking of the mortality from this disease in Spain, it may not be irrelevant on my part to state, for the information of the reader, what I know respecting the intensity of the malady or the type which the disease assumed in that country. With the exception of India, it is probable that no country has been visited more severely than Spain. If a line were drawn from east to west, so as to intersect Madrid, it will be found that few towns to the south of this imaginary boundary, escaped the dreadful scourge, and in its severest form. Of this, Seville and Xeres offer but too melancholy examples; the ravages of the disease, in the former town, putting us in mind of the tales with which we are all familiar, of its irruptions in the east, and from which we have been spared, if not altogether, to a considerable degree in the west, and in Europe.

But to the north of the above line, the disease was less virulent, and confined to two principal routes, extending but to a small extent on either side, and passing over many of the towns situated in the direct line of march which the disease took from south to north.

most probably taken them home and neglected to return them. I was, therefore, still in hopes that they would have been forthcoming before this work had gone to press; but having received no communication respecting them, I do not think it worth while to delay its publication any longer on this account. It will be, however, a duty incumbent upon me to lay those documents before the profession on some other occasion, should I be fortunate enough to recover them, which I still flatter myself will be the case.

On my way to Valencia, as was before mentioned, I touched at Alicante, where the disease was then prevailing; but not being allowed to land and embark again in the same vessel, which I was anxious to do in order to proceed on to Barcelona, where the epidemic had just commenced, I could only obtain one short interview with the practitioners of the above town, several of whom very kindly walked down to the end of the pier, for the express purpose,—an attention for which I have to return my thanks to Mr. Williams, the then acting Consul, in the absence of Mr. Waring, of his Britannic Majesty. During the conversation which ensued, I explained to these gentlemen my object in visiting Spain, and the reasons I had to be so acquainted with the remedy which it was my wish to recommend; leaving them, also, on my departure, a copy of the MS. before referred to which had then been translated into Spanish, but not published.

During a second visit to Alicante, some months afterwards, I learned that the plan of treatment then recommended had been adopted in both the hospitals established there; and that it was principally trusted to for the cure of the disease, during the latter part of the period of its prevalence. That it was found successful may be presumed, when it is stated that, in one hospital, in which previously the powders of Viverra had been almost wholly trusted to, no sooner were the effects of the carbonic acid witnessed, than the above remedies were abandoned, and their place supplied by effervescing draughts. This is no small proof of the efficacy of the latter remedy, more especially when, in addition to what has been already advanced respecting the good effects of these powders, it is added, that a commission of medical men was then in Alicante, having arrived from Murcia with the express view of making known their virtues, and recommending their adoption.

The full benefit, however, to be derived from the carbonic acid could not always be obtained, on account of the difficulty there was in procuring the carbonate, or rather the bi-carbonate of soda

and potash, the sub-carbonate being the only preparation met with in Spain. Although prepared for the occasion by some chemists in this and other towns, as was to have been presumed, the sub-carbonate was frequently substituted for it, and the medical attendant has discovered, when too late, that the patient had been only taking a simple solution of tartrate of soda or potash, instead of a mixture of carbonic acid gas.

Although the Cholera has only prevailed in Spain during the past year, in a few towns in the north of Catalonia, I have been gratified by learning that the carbonic acid was employed in one of them, and with its usual success; as may be gleaned from the following extract of a letter inserted in the *Vapor*, newspaper of Barcelona, No. 261, announcing the cessation of the disease as an epidemic, and the removal of all restrictions—the *Te Deum* having been previously sung. It concludes thus:—"The plan of treatment recommended by Dr. Parkin, whose treatise I obtained, out of mere curiosity, when I was "in Barcelona, has effected wonders, since it has cured all with whom "it has been tried. Would to God," exclaims the writer, "that it "had been adopted, even by one of our Physicians, at the commencement of the disease, to the same extent, as was the case afterwards, "for we should then, most assuredly, have had no victims."

Rosas, Sept. 12, 1825.

